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ANALYSIS OF A ROOF TRUSS.

By Prof. DeVOLSON WOOD.

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In the course of my instruction frames like Figs. 1 and 2, have occasionally been

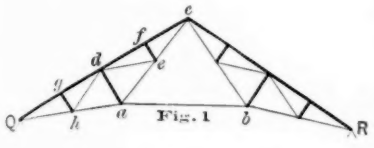


Fig. 1

analyzed graphically. It will be seen that the chief difference in these frames consists in the position of the horizontal tie; in Fig. 1 connecting the points a and b of the secondary truss, while in Fig. 2 it joins the lower ends of the long rafters. In Fig. 1, after finding the reactions of the supports, the analysis proceeds without difficulty until the joint a or d is reached, where three unknown stresses meet. To pass these points various methods have been devised.

This frame is analyzed in the Graphical Statistics of Du Bois, and also in his later work—"Strains in Framed Structures;" and I remember seeing a solution, some years since, in an English journal, I think in *The Engineer*, in all of which there is a resort to a special artifice. In the two

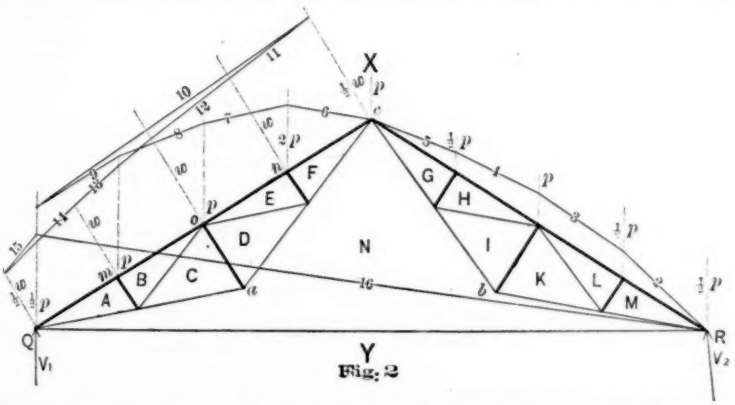


Fig. 2

former works, the stress on de due to the load at f is found, which reduces the un-

known stresses at d to two, after which the analysis proceeds without difficulty. In Fig. 2, three unknown stresses meet at Q and R, so that the analysis of internal stresses cannot begin at these points unless the stress on the horizontal tie be first found. This frame may be analyzed by finding the stress on the secondary parts first, and thence passing to the primary parts; but we have generally sought the stress on the horizontal tie directly. Various methods were resorted to, until, at last, I conceived the idea of using the equilibrium polygon, for this purpose, which gave not only this stress, but also the stress at the apex c. I can hardly presume that this use of the equilibrium polygon is new, although I have no knowledge of its having been so used by others. The process is so distinct, and the results so satisfactory that we have used no other method since it was devised.

For the purpose of illustrating and explaining the principle, I have assumed that the frame in Fig. 2 is loaded eccentrically, by weights $\frac{1}{2}\,p$ at Q, p at m, p at o, $2\,p$ at n, p at c, and so on with $\frac{1}{2}\,p$, p, $1\frac{1}{2}\,p$, $\frac{1}{2}\,p$, the last being at R; and also a wind pressure normal to Qc of $\frac{1}{2}\,w$ at Q and c, and w at each of the joints m, o, n. $n=\frac{1}{4}p=1{,}000$ lbs.

Assume that the frame is hinged at R and free at the support Q, then will the reaction at Q be vertical. In Fig. 3, construct a force polygon, or so much thereof as possible, with the known forces; TP for the wind pressures, PU the vertical weights, and a vertical through T to represent the reaction V_1, the length 7 which remains to be determined. From any assumed pole O, draw the radial lines in the well-known way, by means of which construct the equilibrium polygon in Fig. 2, *beginning at* R *the point of application of the force* V_2, *whose magnitude and direction are unknown*. The radial line 1, Fig. 3, does not appear in Fig. 2, for it cuts V_2 and $\frac{1}{2}\,p$, both meeting at R. The other lines are numbered the same in both figures. The last line in Fig. 3 is 15, but in Fig. 2 the polygon must close, which is done by drawing 16, after which 16 in Fig. 3 is drawn parallel to that in Fig. 2. The line 16 in Fig. 2 cuts V_1 and V_2; hence S Fig. 3, where 16 cuts the vertical TS, is the common point of V_1 and V_2; and TS will be V_1, and US will be V_2 in magnitude and direction. TPUST is the polygon of external forces.

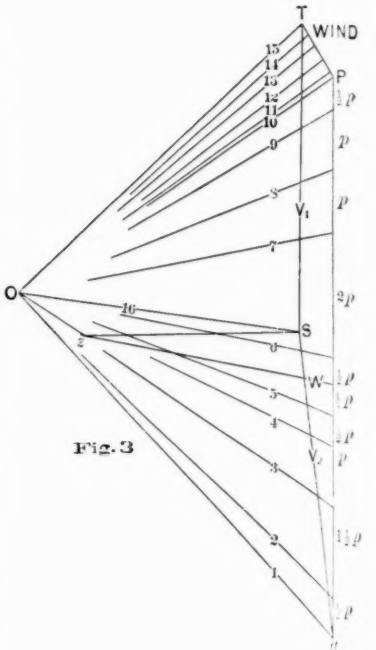


Fig. 3

The magnitude and direction of the force at the joint c is unknown; to find which conceive a vertical plane passed through the point and the left half of the frame unmoved. Make, as before, as much of a polygon of forces as possible with the forces known as acting on the right half of the frame—which are $\frac{1}{2}\,p$ at c, then $\frac{1}{2}\,p$, p, $1\frac{1}{2}\,p$ $\frac{1}{2}\,p$, which last is at R. and V_2 previously found. The several weights p are laid off from W to U Fig. 2, and US$=V_2$ as before found, and SZ of undetermined length, horizontally through S will represent the direction of action of the stress in the horizontal tie. Using the same pole as before, construct an equilibrium polygon, *beginning at* c *the point of application of the force whose magnitude and direction of action are unknown*. In this case it so happens that the first equilibrium polygon passed through c so that the last named polygon will coincide with the former in part. It will begin at c and end at R; hence the

closing line of the polygon will be Rc, coinciding with the main rafter; and OZ Fig. 3, parallel to Rc will intersect the horizontal force at Z, therefore ZS will represent the stress in the tie RQ. The

Had the pole O, Fig. 3, been taken at Z, the equilibrium polygon, Fig. 2, would have passed through c and terminated at Q so that the closing line 16 would have coincided with QR, and in Fig. 3 would

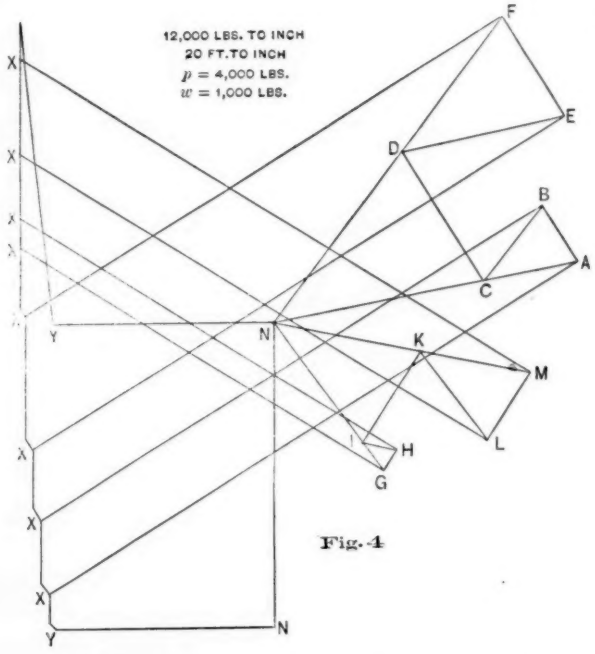


Fig. 4

line ZW, joining Z and W, will represent the stress at c in magnitude and direction. The force polygon for this half of the frame will be WUSZW. Had the other half been taken, the force polygon would have been TPWZST, or the same result would have been found.

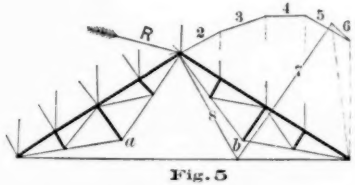


Fig. 5

Now the analysis of the other internal stresses may begin at Q,R, or c, and proceed in the usual way, the results of which are given in Fig. 4.

have been ZS; in which case the stress in the tie QR, the oblique stress at c, and the two reactions would have been given by one construction.

If the first equilibrium polygon, starting at R, does not pass through c, it will be necessary to construct another. Figures 5 and 6 show such a construction.

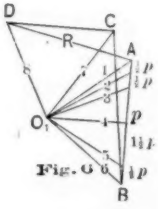


Fig. 6

These are for the same problem as Fig. 2, but the scale of the figures are one-

fourth the size. In Fig. 6, AB=WU of Fig. 3; BC=US; but the pole O_1, Fig. 6, is in a different relative position. The equilibrium polygon 2, 3, 4, 5, 6, 7, of Fig. 5, corresponds with the pole diagram of Fig. 6. The line 8 of Fig. 5 is the closing line of the polygon, parallel to which is 8 of Fig. 6, and the latter by its intersection with CD determines the point D. Then will DC=ZS of Fig 3, and DA=ZW; and the remainder of the solution will give Fig. 4.

ON THE ALTERATION OF MINERAL COAL BY EXPOSURE TO THE ATMOSPHERE, TOGETHER WITH A NEW METHOD TO PRESERVE IT.

By FRANZ POECH, ant. Mining Engineer, Vienna.

Translated by JOHN P. WISSER, First Lieutenant, First Artillery, U. S. Army.

(Oesterreichische Zeitchrift für Berg- und-Hüttenwesen, May 31, 1884.)

It is well known that mineral coal, especially the less compact lignites, holding much water, crumbles gradually by exposure to the air for some time, and the finest and most valuable pieces are gradually converted into rubbish and dust. Bituminous coal is less liable to crumble, but loses by exposure in calorific power, and those varieties applicable for the production of coke lose the power of coking. Most mineral coals have a tendency to ignite spontaneously, and many a coal depôt has already been destroyed by this action.

An effectual method for the prevention of alteration and the spontaneous combustion of coal would therefore be of great value to coal producers, who are often compelled to store coal in enormous masses.

How sensibly the value of coal is dimished by long storage is shown by the following example:

From a heap of stored Bohemian brown coal, medium coal I, which lay exposed 5 weeks, only 60 per cent. of this grade could be obtained in the re-sorting before shipping; the other 40 per cent. was converted into nut coal and dust.

The 40 per cent. removed by alteration lost in value $1\frac{5}{6}$ kreutzer* per hundredweight, from which the calculated loss for the entire quantity of stored coal amounts to 4.3 kr. per hundred-weight, the cost of re-sorting included.

The loss to coal works, caused in this way, is therefore considerable; a work in north-west Bohemia hardly makes 4.3 kr. per hundred-weight of coal. Moreover, the loss increases in direct proportion to the time of storage, and cases may be cited in which 80 to 90 per cent. of the coal deposited was altered by exposure.

Mining-Engineer Wenzel Poech, in Karbitz, has discovered a simple, cheap, and practicable means of preserving piles of coal. It consists principally in leading steam into the coal pile, thus excluding the air and moistening the coal uniformly.

Before proceeding to the description of the process I will discuss briefly the chemical and physical phenomena attending the alteration and spontaneous combustion of coal.

Porous bodies, it is well known, have the property of absorbing gases. This is seen in platinum sponge, which condenses so much air in its pores that the heat produced will cause hydrogen, directed against its surface, to combine with the oxygen and form water. It is also seen in charcoal, one of the most porous substances, representing, as it were, the skeleton of the wood mass. According to the experiments of Saussure, charcoal absorbs 9 times its volume of oxygen, and 35 times its volume of carbon dioxide.

Mineral coal also possesses a porous structure, and Saussure has shown that it absorbs three times its volume of oxygen.

According to Fayol,* the absorption

* 1 Florin (Gulden) =100 kr., is now worth about 42 cents.

* Comptes rendus de la société de l'industrie minérale, 1882, page 74.

power of all coal, from anthracite to lignite, is 5 to 10 per cent. by weight.

The property possessed by porous bodies of absorbing gases is a surface action. The walls of the pores attract the gases, just as water is drawn into narrow tubes. This has not been proven conclusively, but there is good ground for making the assumption; moreover, the hygroscopic power of a substance is taken as the measure of its absorbing power.†

While all other gases absorbed by coal remain unaltered, oxygen is condensed and combines with the carbon and hydrogen, producing heat.

Varentrapp‡ has shown that brown coal oxidizes in a current of air of less than 50° C. temperature, and that the amount of oxygen absorbed, although small, is nevertheless increased by an elevation of temperature, and may result in the complete consumption of the carbon without producing ignition.

E. Richters§ determined the fact that fresh, dry bituminous coal will absorb a considerable amount of oxygen in a few days, without giving off any carbon dioxide and water, and that these oxidation products remain condensed in the pores of the fuel. The latter circumstance offers no impediment to the absorption of oxygen, this only ceasing when the disposable hydrogen and the easily oxidized part of the carbon are combined with oxygen.

At normal temperature the absorption of oxygen proceeds until the complete oxidation of the easily oxidized constituents; hereby carbon dioxide may be given off, but at higher temperatures both carbon dioxide and water are driven off.

In case the absorption takes place slowly, so that the heat produced may be disseminated, and no appreciable rise in temperature results, hydrogen and oxygen remain condensed in the pores; only in the contrary case are they expelled, and new oxygen absorbed, new quantities of heat produced. Through the increase of temperature, however, an energetic chemical action takes place between the oxygen and the combustible material of the coal, the result of which is finally spontaneous combustion.

Coal dust does not absorb more oxygen than coarse granular coal, but the absorption, in consequence of the larger surface presented by the dust, proceeds much more rapidly, and herein, together with the difficulty of furnishing the coal dust with sufficient fresh air to carry away the heat, lies the cause of the more easy spontaneous combustion of coal dust.

According to Fayol the temperature of ignition of lignite, in the form of powder, is 150° C., of anthracite, 300° C.

The causes of the crumbling of coal by exposure are manifold. In all probability it is due to the absorption of oxygen and the resulting carbon dioxide, which, being compressed, must exert a pressure on the walls of the pores. Possibly, also, by the chemical reactions, a body is disturbed which hitherto acted as cement.

Of great importance, directly or indirectly, in this connection is the circumstance that the hygroscopic moisture evaporates after a certain time, leaving the fine cracks and pores free for the penetration of the oxygen of the air, for which there is thus furnished a larger surface for its action.

Now, the escape of the hygroscopic water cannot take place without the production of mechanical alterations, as may be seen in the similar action of the warping of drying wood, or in the cracks of a dried swamp; hence the cracking and crumbling of stored coal through chemical action will be facilitated if the pores are first deprived of their protecting water.

A means which prevents the escape of the original mine moisture must be of the greatest benefit in preserving it. As will be shown more in detail further on, this can be accomplished in a simple manner by immersing the coal in water, or in an atmosphere of steam, since the mechanically held water will not escape in this case, and the passage of oxygen into the pores be prevented. By keeping the coal constantly moist or by steaming it, aside from the fact that the air is also externally excluded from the pieces of coal, the oxidation and alteration may be effectually opposed.

Brown coal loses little in calorific

† Dr. F. Muck, Grundzüge und Ziele der Steinkohlen chemie, page 81.
‡ Johresberichte der chemie, 1865, page 837.
§ Johresberichte der chemie, 1869, page 1120.

power by crumbling, but considerable in commercial value. In bituminous coal, as a rule, the yield of coke in coking, and of gas in dry distillation, suffers considerable loss. Gas works prefer the newly-mined coal, as is quite natural, since the disposable hydrogen, so valuable in gas making, is the first to be oxidized by the absorbed oxygen.*

In regard to the part played by iron pyrite in the spontaneous combustion of coal, the following remarks may be made:

The view that only coals containing pyrite or marcasite are subject to spontaneous combustion, must be abandoned, since the observation has been continually made that often the coals free from pyrite have the greatest tendency to spontaneous combustion.

Fayol, who studied this question thoroughly and conducted careful experiments, remarks in this connection:

1. The first and greatest cause of spontaneous combustion is the absorption of oxygen by the coal.

2. The most favorable conditions for the self-heating of coal are a mixture of pieces and dust, an elevated temperature, a large mass of coal and a certain volume of air.

3. Large pieces, low temperature, small volume, and the complete absence of air or good ventilation, act in opposition to self-heating.

4. In general pyrite plays only an unimportant, mostly insignificant part in spontaneous combustion.

5. Mechanical forces, or the heat produced thereby, cannot be assigned as the usual causes of the burning of coal mines.

Durand assumes that when the sinking roof of a gallery concentrates the entire pressure in a few places, considerable elevation of temperature may be thereby produced.

In order to investigate the effect of pyrite in elevating the temperature, Fayol carried out many experiments by placing pulverized coal, coal and pyrite in an air bath at 100° to 200° C., allowing them to remain some time, and observing the increase in weight and temperature.

He succeeded in showing that pyrite oxidizes less easily than coal (in the air bath at 100° C. the samples of coal took up 7 per cent. oxygen in three months, pyrite only 4 per cent.), and that the addition of pyrite to the coal even retards the increase in temperature. The results of the experiments of Fayol confirm the older work of Richters, but are less complete, in that they have reference only to the air-dried condition of the coal.

That the relation of pyritiferous coal to oxygen is different in presence and in absence of moisture, the experiments of Richters have conclusively proven.

This investigator found by direct experiment that pyritiferous coal, kept constantly moist, absorbs twice as much oxygen as in the dry state. The moisture accelerates the decomposition of the pyrite, and induces further chemical processes, such as the formation of salts, the absorption of water, etc., by which again heat is produced. In order that the moisture may cause this action a sufficient amount of oxygen is necessary.

Moisture, as a rule, rather retards than accelerates the production of heat, and only under peculiarly favorable conditions, and in presence of much pyrite and a sufficient volume of air is it capable of promoting the alterations.

Hence, neither pyrite nor moisture nor mechanical power appears capable, in general, of causing the burning of mines, and, in by far the greatest number of cases, the cause of spontaneous combustion can only be found in the absorption of oxygen.

If the coal is placed in an atmosphere of water vapor, which excludes the atmospheric oxygen from the coal, the hygroscopic water will have no tendency to leave the pores of the coal, nor can a chemical action set in, even in presence of pyrite, the oxidation of which is, under other circumstances, essentially promoted by the presence of moisture.

It is therefore not to be doubted that, by displacing the oxygen and keeping the coal moist, alteration and spontaneous combustion may be checked. A complete immersion would meet the requirements, but would only be practicable in rare cases; irrigation alone would not be perfectly successful.

Wenzel Poech excludes the air and produces a uniform wetting of the stored

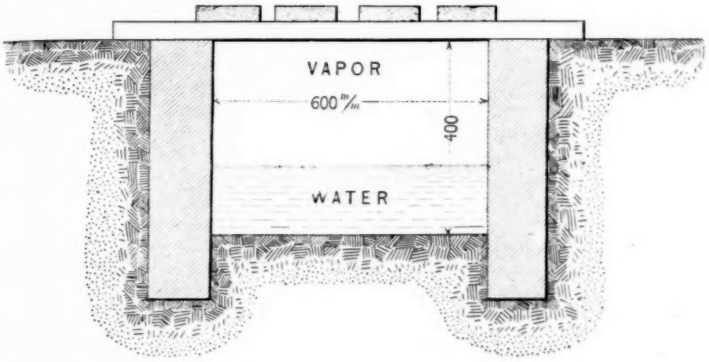
* The "Journal fur Gasbelenchtung und Wasserversorgung," 1884, page 232, cites a case in the Nettlesworth coal, which, after six months' storage, had crumbled to dust. The yield of gas fell from 29 m^3 to 27.

piles of coal, as already mentioned, by admitting spent steam into the depôt pile.

For this purpose a series of trenches are cut in the depôt ground, as shown in cross section here; they are so covered with beams and boards that narrow spaces remain, not large enough to permit the coal to fall through.

coal to be stored and the height of the pile; for medium-sized coal the distance between the canals, with a height of pile equal to 3 m. is 3 m.

The exhaust steam of a steam-engine of 4*e*, which was worked for but six hours during the day, was entirely sufficient for the preservation of a depot of 20 car-loads of coal.



The boards are simply laid on cross pieces, are not fastened, and can be easily removed for the purpose of cleaning out the trenches.

On the ground thus prepared the coal to be stored is deposited in the usual way, the trenches are then connected with the exhaust-pipe of a steam-engine, and the steam admitted; it passes through the interstices in the covering into the coal pile, disseminates itself through the latter, displaces the air, and in consequence of the condensation of the vapor moistens the coal uniformly.

In order to effect a uniform distribution of the vapor it is necessary to cover the coal depot with fine coal and cinders, as in the case of charcoal heaps; thereby strong draughts of air will also be prevented from passing through the pile and interfering with the equal distribution of the steam. In case of coal containing little pyrite, careful covering of the pile is not so necessary, but is of importance in case of coal rich in pyrite.*

The width of mesh of the network of canals which exists in large depot grounds depends upon the size of the pieces of

In carrying out the process it was repeatedly shown that the losses sustained in the purely mechanical movements, in the pouring out at the unloading of the coal, are far smaller than they are usually assumed to be, and that in this assumption a large portion of the waste produced by alteration was attributed to the destruction by the pouring out.

In the loading of the coal preserved by steam it was found invariably that only in the locality where the first unloading took place, and where the coal fell from greater heights, attrition took place, the rest of the coal was good, was well preserved, and could be loaded without re-sorting.

The cost of construction of the trenches and their coverings—for which latter old and otherwise useless boards, beams and road-beams of the mine are utilized—for a depot for 100 car-loads of coal amounts, at most, to 30 florins.

The cost of working is nothing, and the outlay consists only of the cost of preserving the trenches, and the interest and amortization on the applied capital.

Results relative to the use of this process have been obtained at the following works:

* This should be confirmed by determining first whether the covering of the pile, by preventing the escape of the heat developed by the condensation, may not be harmful.

"Exc. gräflich Nostiz'scher Maria Antonia" shaft, near Karbitz.

"Ferdinand" shaft, near Teplitz.

"Bruno" mine, in Weisskerchlitz, near Teplitz.

At the Maria Antonia shaft, in consequence of the thankworthy permission and support of "Bergdirector" J. Neuber, the manager, Wenzel Poech was enabled to make the first experiments, and since then the process has been in full working order.

At the Ferdinand shaft, belonging to the Austrian Coal Industry Association, confirmation of the described preservation method was quite accidentally found; over a reservoir, covered with debris, and into which exhaust steam passed, coal had been deposited, which, on reloading it, was found to be perfectly preserved.

According to communications from mining engineer Hans Gutmann further experiments were carried on with favorable results, and the working of the process on a larger scale was proposed.

The Director of the Bruno Mine, Em. Baier, uses old gratings to cover the canal; he also subjected coal, without covering with cinders, for more than two months to exhaust steam, and found it in no way altered.

To insure a uniform production, and with reference to the increasing losses at the works, it is much to be desired that this process may prove completely successful, especially as the methods formerly used proved worthless.

The usual insertion of wooden tubes and the production of canals in the coal pile is directly detrimental, since oxidation is thereby only favored; investigation has also shown that at the surfaces of contact between the walls of the tubes and the coal, the latter is most liable to spontaneous combustion, so that now, on the contrary, the tendency is to level the coal as compactly as possible.

I arrive, therefore, at the following conclusions:

Freshly-mined coal, deposited on the rubbish piles is capable of condensing several times its volume of oxygen in its pores. The oxygen absorbed enters into chemical combination with the easily oxidized constituents.

According as the absorption is rapid or slow, a greater or less elevation of temperature is produced. In the former it may lead to spontaneous combustion.

The crumbling of coal is, among other causes, a consequence of the absorption and condensation of oxygen in its pores, and the chemical changes taking place. The escape of the hygroscopic moisture favors the absorption of oxygen.

The pyrites can only produce a furthersome effect on the increase of temperature when present in considerable quantities, and then only in presence of moisture and air; in the dry state they must be regarded as perfectly passive, and may even be detrimental to the warming.

Freshly-mined coal placed in an atmosphere of steam can suffer no change. Even with incomplete exclusion of the air the steam will, in general, oppose oxidation and warming, principally by uniform moistening of the pieces of coal.

THE CONSTRUCTION OF IRON VESSELS.

From "Iron."

THE following paper, on "The Construction of Iron Vessels and the Board of Trade Rules in regard to the Safety of Ships and the New Shipping Bill," was read on May 8, at the Inventors' Institute, Lonsdale Chambers, 27 Chancery Lane, by Mr. S. J. Mackie, C. E.

If we regard the whole of the controversies which have been so hotly waged between the President of the Board of Trade, the shipowners, the shipbuilders, and the mariners, can we rise from the contemplation and feel that the roots of the evil have been really reached? Or, that the responsibilities tried to be thrown on shipowners are such as ought not to be properly shared at least by the Board of Trade? The position of shipowners with regard to governmental control ought to be either entire responsibility within well-defined limits, or entire absence of responsibility by compliance

with governmental requirements. Whatever the shipbuilders have done amiss, the Board of Trade has done worse! In fact, in this case of safety of ships at sea, the Board of Trade rules have prevented safe ships from being designed, and have offered a distinct premium for the building and sending to sea of unsafe and untrustworthy ones. Let the Board of Trade revise and alter its own rules, and there will be little need for other legislation. It will not be in the interest of shipowners to own dangerous vessels, if they can get safe ones to be built upon anything like equal terms. The severe inequality of the existing terms are solely due to the rules of the Board of Trade. There are points to be brought out. What really is the condition in which we find the ships of our mercantile marine? Look at the newspapers from day to day under the heading of "Wrecks and Casualties," and for the various accounts of vessels run down in collisions or broken up on going ashore, or capsized from shifting of cargo! Ought an iron vessel to go to pieces when she goes ashore? Or, if properly constructed, ought she not to rest there, and resist the battering of winds and waves until a high tide admits of her release? Ought an iron ship to sink in so few minutes after a collision on the broadside that there is not time to rescue even her crew—still less time to rescue her passengers if she carries any? Ought any sailing ship to capsize if properly designed? Ought a steamer's shaft ever to break by twisting, or engine bearing ever to get hot, as they frequently do, through the strains of a badly constructed hull? These are questions which a knowledge of existing inventions would speedily reply to, and are conditions which, if required to be fulfilled, inventors would quickly answer! Let us go no further than the Thames, and we shall see river steamers built to the requirements of the Board of Trade, and approved by surveyors, having the middle body entirely undivided, with all the dead weight of the engine in it, and only ¼-in. iron skin to keep out the water! Such a vessel being hit amidships, what would be the result? We know it by the Princess Alice. The dead weight of the engines and boilers would tend by gravitation to sink; but the buoyancy of the ends would cause upward pulls, the combined action tending to increase the gash and to pull the hull asunder. Is this a fit vessel to carry passengers by the hundred on a river busy with tidal, steam, and sailing traffic? Common sense says "Certainly not." But the Board of Trade sanctions such steamers, not only for the river, calm and smooth, but for service on the boisterous seas! Does anyone conceive that the surveyors of the Board of Trade don't know such vessels to be unsafe? Assuredly they must know it; but will the Board of Trade permit the interference of its officers? The Board of Trade declines responsibility, but it does not decline to enforce its antiquated rules upon the designers of ships—rules neither founded on arithmetic nor honesty, but which fudge the tonnage of the ship, and risk the sailors' lives, to save the owners' pockets in dues to be paid? Are not harbors and lighthouses, whether English or foreign, worthy of their tolls for their services to shipping without being cheated of their dues by rules of measurement, which give dangerous vessels the advantage over safe ones? These rules have been handed down from our grandfathers, who had wooden vessels, and kept the carrying trade of the world in their hands. So it is that still, by the breadth and half breadth and other complications, the Board of Trade rules bring about the calculated result, that a rectangular box swum one way in the water will carry more upon the same displacement of water, than it will if swum the other way—which is nonsense. In the olden time it gave the shipowner a governmental certificate far below the carrying capacity of his vessel, and consequently his ship's dues were less than they ought to be whenever he had to pay them. These rules do the same thing now for the modern iron ships and the modern shipowner. But note what has been the effect of these rules upon the naval architect, the designer, and the contractor! The ancient shipowner regarded that man as the best architect who could produce a sea-going vessel which could carry her cargo best on the same tonnage measurement by Board of Trade rules. Hence we have in the stated details of vessels, "tonnage registered" as distinct from "displacement." The naval architect has almost disappeared. Ship-

builders have become ship designers, and cheap ships have become as cheap houses. Shipowners think most of the firm who can get most cargo into a ship of the same registered tonnage. It is only to know what is the fundamental principle of this dodge to comprehend at once how its application must produce the worst sailing and the worst steam vessels, as well as the most dangerous, the more fully it is employed and carried out. There is no form of vessel or receptacle that will carry more than a square box. Hence the direct outcome of the Board of Trade rules is the square midships section. Now, floated in the water there is no form more unstable. It capsizes naturally, and load it uniformly, or how you will, the tendency to capsize is always there. It is innate, and can never be got rid of. The square midship section, then, is the dodge—the instability of the ship is the result. The old wooden vessels had a reserve of buoyancy in the thickness and lightness of the material of which they were constructed, and waterlogged wrecks were not uncommon. There are no waterlogged iron, single-skinned vessels, and one reason why steel is preferred to iron is that that material can be prepared thinner and lighter, and so add to the carrying capacity of the hull. But the thinness of the material adds also to the danger of those on board, who have to traverse the seas in sailing vessels. Sailing ships have to be varied in depth in proportion to the sails they carry. The sailing ship practically is a pendulum, the weight upon the bottom of the hull bringing the sails upon the mast against the pressure of the winds. The sailing ship, therefore, must cleave the water as she passes through it. Not so the steamer; the steam engines make it independent of the winds, and its contained motive power can propel it, if not entirely, at any rate to a large degree, over the water, instead of through it. If sail and steam power are combined, the requirements in the form of the hull will have to be modified accordingly. The proper fundamental condition for steamers is breadth of beam. Broader steamers of the same bulk will steam faster than narrow ones. Broader steamers will be more stable than narrow ones, will have less draught of water for the same dead weight of cargo carried than narrow ones, and consequently they can get into tidal harbors, and estuaries, and rivers where deep-draught vessels cannot go. Breadth of beam has also similar advantages in ships with sail and steam power. But breadth of beam is worse than prohibited by the Board of Trade rules. It is absolutely fined! It is charged for dues by the registered tonnage, for more tons than can actually be stowed. But breadth of beam—at any rate, properly proportionate breadth of beam—is essential to the designer, if he is to give proper outline and form to his vessel. He cannot bring out the best, the easiest, the steadiest, fastest, safest, and most seaworthy and stable form of hull without being as unlimited in the scope of breadth as he is in the scope of length. Neither can he bring out all the good qualities of hull if he be restricted in the application of mechanical principles, or in mode of arrangement. All these are completely independent, except under Board of Trade rules, of the proper requirements for tonnage measurement. All that the Board of Trade ought to do is to ascertain that there is buoyancy enough in the particular ship to be registered for her to carry a given weight of cargo upon a given load-line—in other words, that the cargo-carrying capacity shall be estimated by displacement. Thus, say that a thousand tons of cargo, whether in bulk or dead weight, shall be taken on a thousand tons displacement of water, and that the load-line should be fixed and written on the register at this point. Take it another way: the vessel, when light, draws, say, ten feet of water, it is easy to calculate that with a thousand tons' weight of cargo on board she will draw, say, 20 feet of water. Her load-line should be marked at 20 feet and her registered tonnage certified at 1,000 tons. Do this, and the naval architect can do his very best, and the shipowner will be able to buy a safe ship for almost the price of a dangerous one. He will then pay any small extra cost, as he will save it in insurances at diminished rates, as he can have a vessel which cannot founder, even if cut in two! He will have an easier ship, a faster ship, one that cannot capsize, and he will have no more dues to pay than he ought to do,

and which it is right and proper and honest that he should pay. The accommodation for passengers on this ship will be better than on a narrow one. The stowage of cargo will be better, and the lives of the captain and his crew will be unimperiled by any of the needless and terrible dangers brought about by the senseless rules of the Board of Trade, and its still further objectionable proceedings of allowing the owner to fix his own load-line disc where he pleases upon the ship. The introduction of iron into shipbuilding has been effected by men who were imbued with the notions of wooden sailing-ship builders. Hence the semblance of carpenters' work still remaining in iron ships. The old leaven leavens the mass, sailing ships and steamers alike. The practice of the shipbuilders designing their own ships is a grave barrier to progress in the general improvement of iron vessels. The shipbuilder now generally builds the engines also, and consequently there has arisen the convenient but not incontrovertible theory that the form of the vessel has nothing to do with the speed, and hence we are told that "you can drive any form of vessel at any speed you like, only you must put in the horse-power, and pay for it."

What, then, are the essential requirements for an ideal proper iron ship? (1) Strength, combined with lightness of construction. (2) Safety, by division of the hull by bulkheads, both transverse and longitudinal. (3) Buoyancy sufficient to float the vessel with its cargo properly, and enough to spare as a reserve to prevent the vessel sinking under any amount of damage she may receive afloat. (4) Rigidity in the construction of the hull, so that there shall not be contortions and vibrations of its frames and structure, and that the bearings of the engines and shaft shall not be subject to strains detrimental to the working of the engines. (5) Proper design of ship, so that the vessels shall have the best qualities of easiness, stability, and external form. Seeing the weakness of existing vessels, it is clear that the longitudinal strengthening of the ordinary hull is a most vital matter. Regarding the box girder as the strongest form of iron construction, I have always advocated as the fundamental basis of an iron ship the assumption of this particular rectilineal form. This is easily accomplished, and to any extent required, by the disposion of the transverse bulkheads with an iron deck and the introduction of two longitudinal girders. By rising the kellon internally the box girder chambers, into which the interior of the hull is thus converted, is supported for its whole length, and in this way an end-on-end strength is imparted to the ship as should bear her harmless from serious effects from end-on-end encounters. The longitudinal girders would be of even far more importance against broadside collisions. We have seen the Arona traverse the ocean for hundreds of miles with her bows stove in by collision with an iceberg which would not get out of the way; and we have read of the Vanguard going down in fine weather and in broad daylight from a tap amidships. Amongst merchant vessels, with scarcely any exceptions, iron vessels of all classes go down after broadside collision so suddenly that only a few out of the many on board are saved, even when brave hearts and willing hands are ready to do any deed of daring if but a few minutes of precious time were permitted for the exercise of their courage and their help. By the combination of longitudinal and transverse bulkheads, only one or two local compartments, external to the main and vital interior box girder chamber of the ship, would be filled with water in the most severe collisions, and the sinking of vessels from broadside collisions would be thus eliminated from the category of the serious dangers of the sea. The introduction of longitudinal girders would prevent any dangerous shifting of cargoes, and would also—under a proper Board of Trade system of registration for dead weight of cargo to be carried—a sufficient reserve amount of buoyancy, reserved, too, in the right places, as should, under every circumstance, preserve the vessel from sinking. Such spaces of reserved buoyancy should not be legally used for cargo. They would not remove or take away from the buoyancy ordinarily in action for the flotation of the ship and its cargo, but they would come into action when, and only when, the ordinary buoyancy was interfered with, whether by intake of water by breach of the skin of the ship, or by

the heeling over of the vessel from sail power, or from any other cause. Then the reserved spaces would exert a righting influence automatically, and in direct proportion to requirement. Some of the practical officers of the Board of Trade have personally recognized the value of this system of designs which I have unofficially submitted to them, as also have various officers of the great insurance companies. Some of the most eminent of our best shipbuilders have also been willing to contract for building such vessels as against estimated price, and to take the risk upon themselves of repayment of cost as against very high speed to be realized. But how could the invention progress when the shipowner was subject to loss under the Board of Trade rules for every improvement which he was asked to make, and which, it is true, added something to the cost of the construction of the ship. Even the total extra cost was in some measure due to the unalterable requirements of the Board of Trade, which prevented their surveyors from passing vessels for classification with an external skin of less than a certain thickness, and which thickness might be right in regard to single-skin ships of ordinary construction, where the main strength of the ship is in the skin; but which rules are clearly inapplicable to vessels constructed with an internal box girder chamber, constituting an independent citadel of strength and safety, not to be compromised by the damage or even total loss of the outworks. The Board of Trade have already, on the application of Messrs. Denny, of Dumbarton, eliminated the double bottom from measurement for the registered tonnage; why not go the whole length further and eliminate buoyancy spaces?

Whilst vessels are measured from outside to outside, and the whole cubic contents calculated by the existing rules of the Board of Trade, so long will the rectangular midship section of hull be adopted by shipowners. But it is, as has been said, an unstable form, and water ballast is required to keep the vessel from turning over. The more dead weight cargo that is put in, the better for the stability; but it is the reverse with homogeneous or bulky cargoes, such as grain, cotton, and so forth. If, after the cargo is taken in, the load-line of the ship is brought out of the water by pumping out the water ballast, the instability of the ship is increased. If double sides be permitted to be exempt from tonnage measurement, their adoption by shipowners will be advantageous to them. If the concession of eliminating the double bottoms was right—and it *was* right—then the first concession should be followed and longitudinal side spaces should be conceded also. By taking the actual load-line of a vessel as representing the displacement of water by actual weight of cargo, no vessel with an improper cargo on board could go to sea unobserved, and the pranks which are played with the Plimsoll load-line at this time would be no longer practicable. The dodges played by shipowners between double bottoms and water ballast and cargoes only require to be made intelligible for the public to understand how valueless, in regard to the safety of our sailors, as well as our ships, have been the ways and doings of our Board of Trade, how utterly inadequate has been the legislation on the subject, and how profoundly ignored by Parliament have been the fundamental scientific principles which govern the vital subjects of naval architecture, shipbuilding, speed, safety, and loading. One of the missions of the Inventors' Institute, it seems to me, should be to watch over discussions of such national importance, and in such cases to bring under the attention of the public and of the legislature the scientific principles involved, and to demonstrate the general practical bearings of existing inventions.

At a recent meeting of the Philadelphia Engineers' Club, President Ludlow described tests of the crushing strength of ice, which were made by him in order to learn approximately the strength required for an ice harbor of iron screw-piles, in mid-channel, at the head of Delaware Bay. Eighteen pieces were tried with Government testing machines at Frankford, Philadelphia, and at Fort Tompkins, Staten Island. The specimens were carefully prepared 6in. and 12in. cubes, and roughly cut slabs about 3in. thick, of different qualities and from different localities. For pure Kennebec ice, the lowest strength obtained was 327 lbs., and the highest 1,000 lbs. per square inch. For inferior qualities, the strengths varied from 235 lbs. to 917 lbs. The higher results were obtained, generally when the air temperature in the testing room was from 29 deg. to 36 deg. Fah., as against from 55 deg. to 68 deg. Fah. for the lower results. The pieces generally compressed from ½in. to 1in. before crushing.

EXPERIMENTS ON THE PASSAGE OF ELECTRICITY THROUGH GASES—SKETCH OF A THEORY.*

By ARTHUR SCHUSTER, Ph.D., F.R.S.

From "Nature."

The passage of electricity through gases has of late years become a very favorite subject for experimental investigation. A large number of facts have thus been accumulated, and it becomes of importance to see whether these facts throw any light on the theoretical notions which we have based on other branches of electrical inquiry.

If we have two bodies at a different electrical potential separated by a layer of air, we might imagine the air in contact with the bodies to become electrified, then move on, impelled by the electric forces, and re-establish equilibrium by giving up their charges. The passage of electricity through gases would then be similar to the diffusion of heat. But, however natural such a view would be, it is impossible to maintain it in the face of experimental facts. The experiments which I shall bring before you to-day seem to me to support, on the contrary, the idea that the passage of electricity through a gas resembles the phenomenon studied by Helmholtz under the name of electrolytic convection.

I shall avoid as much as possible all suppositions and hypotheses which cannot be put to the test of experiment; but it seems necessary to start with some assumption in order to avoid too great a vagueness in the subsequent explanations. The assumption which I shall make is this: In a gas the passage of electricity from one molecule to another is always accompanied by an interchange of the atoms composing the molecule. I shall also try to prove that many facts are easily explained by the assumption that the molecules are broken up at the negative pole.

If, in a vacuum-tube of the ordinary form, the discharge is passed at a pressure of about one millimeter, a luminosity is seen round the negative pole which is called the negative glow. A luminous tongue projects from the end of the positive pole, which I shall call the positive part of the discharge, without meaning to imply that it is charged with positive electricity. The positive part of the discharge and the negative glow are separated by a non-luminous space, which I shall call "the dark interval." The glow itself is divided into three layers, the thickness of which increases with decreasing density. Closely surrounding the electrode itself, we have in the first place a luminous layer, which on new electrodes is of a golden color. The spectroscope shows the presence of sodium and hydrogen; the sodium is due to foreign matter deposited on the electrode, and the hydrogen is expelled by the action of the heat out of the electrode by which it had been absorbed. When the electrodes have been in use for some time, the golden color disappears, and the spectrum belonging to the gas used is seen. The second layer is known by the name of the dark space. The third layer is the glow proper.

The theory which I shall endeavor to establish is this: That within the first layer the gaseous molecules are decomposed, that their negative parts are projected with great velocity through the dark space, that this velocity is gradually reduced by impacts within the glow, and that in the positive part of discharge the discharge takes place by diffusion except when stratifications appear.

According to the kinetic theory of gases, the molecule of mercury vapor consists of a single atom, which is incapable of vibration. Mercury has a very brilliant spectrum, which proves that the theory is incomplete in some important point. It is well known, on the other hand, that the theoretical conclusion receives support from the fact that the vapor-density of mercury vapor is anomalous. If, as is generally supposed, the molecule of the majority of gases contains two atoms, that of mercury can only con-

* Abstract of the Bakerian lecture. Read before the Royal Society.

tain one. If an essential part of the glow discharge is due to the breaking up of the molecules, we might expect mercury vapor to present other and much simpler phenomena than the gases with which we are generally accustomed to work. *This, indeed, is the case; for I find that, if the mercury vapor is sufficiently free from air, the discharge through it shows no negative glow, no dark spaces, and no stratifications.* At the ordinary temperature the spark does not pass through mercury vapor, but if a tube free of air, but containing mercury vapor, is heated, the discharge passes always in a continuous stream of light. It is not always quite symmetrical with respect to the two poles; and a very curious tendency of the spark is noticed, to pass at the negative pole rather from the glass out of which the electrode protrudes than from the metallic electrode itself. A brilliant sodium spectrum then appears at the point from which the spark sets out. Whenever small traces of air remain, stratifications are very apt to appear, as a mixture of air and mercury gives fine stratifications, but I have never noticed them after sufficient removal of the air.

I now pass to the description of an experiment which seems to me to be only capable of explanation by the views brought forward in this paper, and I should like therefore to consider them as crucial experiments, which have to be explained by any true theory of the discharge. As negative electrode, I use an aluminum cylinder of 5.5 cm. internal diameter and 8 cm. long. A long aluminum wire running parallel to the axis of the cylinder at a distance of about an inch formed the positive electrode. On exhaustion, the discharge at first passes as a spark in the ordinary way, but as the pressure decreases the glow gradually surrounds the whole cylinder, *with the exception of a dark strip about* 2 *or* 3 *cm. in width, directly opposite the positive wire.* The positive electrode seems, therefore, to repel the negative glow.

The following seems to me a plausible explanation of the phenomenon which I have just described. The rapid fall of potential which is observed on crossing the negative electrode suggests at once, independently of any theory that we have to deal with, the action of a condenser, for we know that no statical charge can produce a finite difference of potential at the electrode, while a double layer will produce a discontinuity. Although it may not be proved that an absolute discontinuity of potential exists at the kathode, it is yet certain that a very rapid fall occurs at that place. This is all that is necessary for the argument.

We recognize such a double layer in the case of electrolytes, but there is an essential difference in the thickness of the layer within which we must imagine that condenser action to take place. In the liquids that thickness must be very small, as is shown by the intensity of the observed polarization currents The positively electrified matter in every case is kept against the negative surface by a joint action of electrical and chemical forces, for it has been shown by Helmholtz that only thus can we explain a difference of potential between two bodies. It is the chemical forces which keep the electricities asunder. The gaseous molecules or atoms, however subject to their mutual encounters, and always having certain velocities, will tend to leave the surface. They are kept near the surface, however, by the electrical forces.

Suppose, now, that a positive electrode is placed near such a condenser. The resistance of the gas is so much greater than that of the metal electrode that we shall assume the whole electrode to be of the same potential. The lines of force will then cut the surface at right angles, and could we assume the condenser to be infinitely thin, there would only be a normal force acting on its particles; but as the lines of force are curved, the particles not in immediate contact with the surface are acted on by a tangential force which will tend to drive them away from the positive electrode. As a steady state will only be possible when the total force is normal throughout the condenser, we arrive at the condition for the steady state that within the condenser the fall of potential must be the same for equal distances measured along the normal to the surface.

Experimental evidence speaks strongly in favor of such a conclusion. If, for instance, a thin wire is used as electrode, it is well known that the tension at the end of the wire before discharge is very much larger than anywhere else. At high pressures the discharge passes indeed

from the end of the wire, but as the exhaustion proceeds, the glow gradually covers the whole wire, and the same amount of electricity flows out of equal areas situated anywhere on the wire, for the dark space which alters its width with the intensity of current is everywhere the same; this implies that the fall of potential per unit distance is the same all over the wire.

Hitherto we have only assumed a certain number of particles positively electrified in the immediate neighborhood of the negative electrode, and we have left it altogether undecided what these particles are. But if we consider now the fact that the glow does not appear opposite the positive electrode, that is to say, that while the the fall of potential is the same all over the surface the flow is stronger at some places than at others, we are driven to the conclusion that the flow does not altogether depend on the fall of potential, and we must again look for an explanation in the chemical as well as the electric forces. Wherever the fall of potential is chiefly produced by the presence of the positively electrified particle, which I now assume to be the decomposed molecules of the gas, these will help by their chemical action to decompose other molecules. Opposite the positive pole the fall of potential is principally due to nearness of that electrode; chemical forces are absent, and the molecules will not be decomposed. This is, I believe, the explanation of the dark area. And it brings with it the explanation of a large quantity of other facts, as, for instance, the one which has been so long observed and well established, that once a current is set up in the gas it requires a much smaller electromotive force to keep it going. For the discharge, according to us, will generally be introduced by a spark which must give the first supply of decomposed molecules before the continuous glow discharge can establish itself.

I may for the sake of clearness once more mention shortly the principal points of the argument.

The rapid fall of potential in the neighborhood of the negative electrode renders the presence of positively electrified particles in its neighborhood necessary.

If the distance through which the condenser action takes place is sensible, the positively electrified particles will be acted upon by a neighboring positive electrode.

A steady state will be established in which the fall of potential along the normal from the surface will be everywhere the same.

As, however, the flow is stronger away from the positive electrode, we must conclude that other forces besides electrical forces determine the flow.

It is natural to assume that these are chemical forces: that, in other words, the positively electrified particles are the decomposed molecules, which by their presence assist the decomposition of others, and therefore the formation of the current.

Unless a flaw is detected in this line of argument, I think that the conclusion must be granted, namely, that the decomposition of the molecules at the negative electrode is essential to the formation of the glow discharge. This is really all that I endeavor to support in this paper. The rest can only be settled by further experiments. And amongst the rest I count also the primary cause which originally produces the decomposition of molecules at one pole rather than at another. It is possibly due to an electromotive force of contact between the gas and the electrodes which tends to make the gas electro-negative.

The gaseous molecules, then, according to our theory, are decomposed at the negative pole. Their negative constituents can follow the electric action, and as the fall of potential in the immediate neighborhood of the pole is very rapid, the atoms will leave the pole with considerable velocity. That the region of the dark space is filled with matter projected from the negative pole follows almost conclusively from the experiments of Goldstein and Crookes, and is also shown in a most striking way by an experiment due to Hittorf. If a tube contains two parallel wire electrodes at a distance of, say, a quarter of an inch, the discharge will at high pressure pass in the usual way from electrode to electrode, but at very low pressures the discharge from the positive pole goes away from the negative. The results can be shortly expressed by saying that, as far as the positive pole is concerned, the inner boundary of the dark space forms the negative electrode. If the dark space is small and does not

reach to the positive pole, the discharge passes from the latter towards the negative pole, but as soon as the dark space extends beyond the positive pole, the positive part of the discharge goes towards the nearest point of the dark space that is straight away from the negative pole.

We have then two closely adjoining, almost overlapping, parts, in which the discharge is in opposite directions, and this could not be unless electricity is carried by matter which can, owing to its inertia and high velocity, move against the electric forces. To my mind this experiment proves conclusively that the negative electricity is bound to matter projected with high velocity away from the negative pole.

Goldstein has shown when a thin pencil of the negative glow belonging to one electrode passes close to another the pencil is deflected. According to our view, such a pencil would be formed by a succession of negatively charged particles projected in nearly the same direction away from the negative electrode; as these particles pass by another kathode, they are naturally deflected out of their path by the electric forces. Goldstein has shown that if the current is equally divided between the two kathodes, the deflection is independent of the intensity of the current, the pressure, and the nature of the gas. This is exactly what ought to happen according to our theory, for strengthening the current at one kathode means, as will presently appear, increasing the velocity of the particles. The square of the velocity will increase in the same ratio as the total fall of the potential in the neighborhood of the negative pole; as the particles pass the other kathode, the forces from it are increased in the same ratio as the square of the velocity with which they are moving, and consequently the path will remain the same. Similarly all the other experimental facts established by Goldstein can be easily explained.

The most conclusive proof of the view adopted in this paper would be found in the demonstration that the amount of electricity carried by each particle was always the same, whatever the current. I propose to test this fact in the following way:—It was found by Hittorf that the particles proceeding from the negative electrode, and projected at right angles to the lines of force in a magnetic field are bent round in a circle. This is as it should be, and I calculate that the radius of the circle ought to vary as $\sqrt{F/e}$, where F is the total fall of potential within the region in which the particles acquire their velocity, and e is the amount of electricity carried by each particle. As the current increases, it is shown by Hittorf that F increases; and I find that at the same time the diameter of the ring in the magnetic field increases. If this diameter varies as the square root of F, it would be proved that e must be constant as it is in electrolysis. At present we can only say that the average amount of electricity carried by the particles must increase less rapidly than the fall of potential. If e varies at all, we should expect it to vary proportionally to the fall of potential in the neigborhood of the negative electrode, and in that case the diameter of the ring would be independent of the current, which it is not.

The theory which I advocate involves the existence of a polarization, and it might be considered a difficulty that no polarization currents have with certainty been observed in gases. I believe the difficulty only to be apparent, for the experiments prove that the fall of potential near the negative pole, though rapid, is not sudden, so that the layer within which the condenser action takes place is very much thicker in gases than in liquids. The capacity of the condenser is therefore smaller, and though the total fall of potential in the gas may even be stronger than in the liquid, the polarization currents might escape observation.

With regard to the positive part of the discharge it will be sufficient here to mention that stratifications are principally observed in mixtures of gases or in compound gases, and that in the intervals between two stratifications the discharge is very likely carried as through the dark space at the negative electrode, while in the stratifications re-combination of the decomposed atoms takes place.

An interesting law has been proved by Hittorf and E. Wiedemann in the case of the unstratified discharge. Hittorf shows that the fall of potential is the same in the positive part for the same tube whatever the current. This means that the

energy dissipated is proportional to the current, and not to the square of the current as in a liquid. In the latter form the proposition had previously been proved by E. Wiedemann, who has shown that the total quantity of heat generated is proportional to the total quantity of electricity which has passed through the tube, whether in a few strong sparks or many weaker ones.

These experiments seem to point to the fact that once the original velocity of the particles at the regular pole has been reduced the velocity becomes independent of the strength of the current, that is to say, that in the positive part of the current greater intensity only means a greater number of particles taking place in the discharge.

The paper also contains spectroscopic evidence as to the state of dissociation in a vacuum tube, especially in the negative glow.

The question as to how the electricity passes from the electrode to the gas is not discussed, nor is it possible at present to decide, should the theory prove true, whether the polarity of the atoms in the molecule depends on the way in which these are combined, or whether that atom takes positive polarity which happens to be nearest the negative electrode as the molecule approaches it.

In conclusion some novel influence of the magnet on the negative glow is described, and it is shown that two different effects have to be clearly distinguished. The first is an effect of the magnet on the discharge when that discharge is established, and has been sufficiently well investigated. But the second effect depends on the question from what part of the negative electrode the discharge sets out. With respect to this question we meet with many contradictory and inaccurate statements. If at any place the magnet tends to throw the glow together the temperature will be raised, and owing to this fact the current will be strengthened, which again raises the temperature. It may thus happen that a slight cause can induce the current to pass almost exclusively from one part of the negative electrode. For a detailed description the reader is referred to the paper itself and the illustrations accompanying it.

THE TIDAL WAVE AND THE "MASCARET"—IMPROVEMENT OF TIDAL RIVERS.*

By E. SHERMAN GOULD, C. E.

Reviewed for VAN NOSTRAND'S ENGINEERING MAGAZINE.

THERE is perhaps no branch of physical science more interesting than that which relates to the observation of the movements of the ocean and the study of the laws which govern them; and perhaps few departments of applied science call for more genius, or demand higher qualifications than sea-coast and river engineering. There is an awful grandeur belonging to the ocean which rivals in impressiveness the serene stillness of the star-lit heavens, while the triumph of man's intellect in mastering, even if only partially, the laws which the mighty restless mass of waters obeys, seems almost more wonderful than the conquest of the celestial spaces by astronomy. The quiet void of the heavens and the undeviating march of the stars, seem to furnish fit elements for the exercise of an exact science, but how hopeless, one would think, to search for fixed laws in that which is itself the type of instability!

In the volume before us, Mons. Comoy has developed an admirable and comprehensive treatise upon the principal phenomena of wave-motion. Assigning to himself in the first instance the task of studying that curious phase of tidal action known as the *mascaret*, or *bore*, he found himself forced to trace back all the anterior phenomena connected with it, and has so been led to establish the whole theory of wave and tidal motion. This

* *Etude pratique sur les Marées Fluviales, et notamment sur le Mascaret. Par M. Comoy.* Paris, 1881. Octavo, pp. 389. Figures in the text and atlas of 10 plates. Price $5.25.

labor which, in view of the author's main object was but a means to an end, becomes the most valuable feature of the work to the American engineer, because none of our rivers, we believe, present the peculiarity of the mascaret.

Waves may be broadly divided into two classes, namely, Waves of Translation, and Waves of Oscillation.

When a boat with bluff bows is forced along a canal, a certain quantity of water is heaped up under her fore-foot. Should the progress of the boat be suddenly arrested, there would be formed, after a brief period of tumultuous agitation, a wave which would detach itself and roll onward in the direction in which the boat had previously been moving. This wave would be a Wave of Translation. Such waves are formed when a mass of water is shoved forward horizontally by a sufficient force. They move along bodily, extending from side to side of the channel within which they were propagated, widening when the channel widens, the onward motion extending also from the surface to the bottom of the water.

The principal visible characteristic of a wave of translation is that it stands wholly in relief above the adjacent surface, like a hill rising from a level plain.

The velocity with which a wave of translation moves is proportionate to the depth of water in which it is propagated, and its height above the surface. It is represented by the formula

$$V = \sqrt{g\,(H+h)} \mp U$$

in which g=gravity and H and h respectively the depth of water in the channel and the height of the wave, and U the current, if any, in the channel, using the sign + or − according as it runs with or against the direction of the waves.

This velocity V is that of the undulation of the wave: not that of the forward horizontal movement of the molecules of which it is composed. This last velocity, represented by v is given by the proportion:

$$\frac{v}{V} = \frac{h}{H+h}$$

The wave of translation does not reproduce itself: each wave requires a distinct force to generate it. For this reason French physicists give it the name of *onde Solitaire.*

Waves of oscillation are divided into two classes, ordinary and periodic. Of the latter the great example is the tidal wave.

The ordinary wave of oscillation is generated by any force which produces a temporary vertical depression or elevation in the surface of the water. Thus, a stone thrown into the water produces waves of oscillation. They always go in groups, and their principal visible characteristic is a series of alternate elevations and depressions above and below the general surface of the water. The wind, striking the surface of the water obliquely, produces by the action of its vertical component, waves of oscillation.

While the action of waves of translation exercised in the channels of moderate dimensions such as rivers and canals, in which their movements have been studied, extend undiminished from the surface of the water to the bottom, that of the wave of oscillation which has been investigated on a much vaster scale, extends to only a comparatively moderate depth, rapidly decreasing from the surface downward.

This statement must be made, however, under a certain reserve, and must be understood to apply, as regards oscillatory waves, to those of which the length and height are insignificant in comparison with the depth of water in which they are generated. When the dimensions of the waves are great in comparison with the depth of the water, the agitation which they occasion is felt to a greater or less extent through the entire depth.

Periodic waves of oscillation are generated by a series of periodic actions, each one producing a wave; the waves thus produced following each other in regular succession.

It is only the periodic wave of which the movements have been thoroughly investigated.

In deep water, the velocity with which a periodic wave of oscillation is propagated is given by the formula

$$V = \sqrt{\frac{g}{2\,\pi}\,L}$$

In which g=gravity, π=3.1416, and L= length of wave.

If the depth of water be less than the distance below the surface at which the agitation produced by the wave would be manifested if the water were deeper, we

should have, as an approximate formula,

$$V=\sqrt{g\,H}$$

in which H=depth of water.

The above are velocities of propagation, not of translation. It is generally understood that waves of oscillation have no regular horizontal progressive motion, but only an oscillatory movement, as the name implies.

As has been already stated, the tidal wave which twice in each lunar day traverses the ocean spaces of the globe, is the grand representative of the periodic wave of oscillation.

Although this wave is due to the influence of the moon and of the sun, it does not follow the apparent path of these bodies, nor yet coincide with the times of their meridian transits. Thus, the wave coming from the South of Africa, after sweeping around the Cape of Good Hope, rolls to the north towards Europe and bears down upon France and England in a south-easterly direction. Part passes through the channel from west to east, and part goes around the North of Scotland into the North Sea, from north to south. This portion enters the channel from the North Sea, going west, where it meets that portion which, as we have seen, enters the channel from the west.

It would seem that the tidal wave owes its unvarying periodicity to the sun and moon, but once generated by these bodies it is abandoned, as to its future course, to purely terrestrial influences, namely the presence of continents and the greater or less depth of the sea.

The ocean tidal wave is of relatively low height, but immense length and velocity. In seas of great depth, say of 2 miles and over, like the Atlantic, it attains the prodigious length of 5,000 miles. Owing to its great length, we must conclude that it causes motion down to the very bottom of the seas which it traverses.

Owing to the same fact, we have the curious anomaly to encounter, that the wave, in passing over the deepest seas, falls under the classification of an oscillatory wave propagated in shallow water. Its velocity is therefore, as before

$$V=\sqrt{g\,H}$$

Direct observation confirms the correctness of the above formula.

Besides the above velocity of propagation, there are the ebb and flood currents. These currents are more perceptible in proportion as the water becomes shallower.

We may note as one of the remarkable phenomena of the tides, that the turn of the tide does not correspond with high and low water, the ebb commencing before the water has ceased rising, and *vice versa*.

It would be impossible within our limits to even glance at Mons. Comoy's masterly analysis of the various movements of the tidal currents and their causes. Suffice it to say, that he finds them to conform to the *law of least action*, in the exchanges of water necessitated by the undulations of the wave, and that they are exclusively undulatory, having no connection, at least in mid-ocean, with the slope of the surface of the water.

The formula for the mean velocity of the flood-tide is given by the proportion

$$\frac{v}{V}=\frac{h'}{H}$$

In which v=mean velocity of tide; V=velocity of propagation, h' half of the total height of wave, and H=depth of water. By substitution of $V=\sqrt{g\,H}$ as already given, we have

$$v=\frac{h'\sqrt{g}}{\sqrt{H}}$$

for the mean velocity of the flood current.

This formula has been verified by observations in the English channel, near the Pas de Calais.

To calculate the distance traveled by a molecule of water on a flood-tide, Mons. Comoy gives the formula

$$l=\frac{\lambda\,v}{V-v}=\frac{\lambda\,h'}{H-h'}$$

in which l=the distance; v=mean velocity of flood current; V=velocity of propagation of wave; $\lambda=\frac{L}{2}$ half length of wave; h'=half height of wave, and H=depth of water. The length of the wave is given by the formula

$$L=T\sqrt{g\,H}$$

in which T=mean half lunar day in seconds=44,400 seconds.

Mons. Comoy gives some very interest-

ing numerical examples of these formulæ, checked when possible by direct observation, but our limits bar these out, as well as much other interesting matter.

Let us pass to the tides which enter the estuaries of tidal rivers. When the tidal wave of the ocean passes in front of the mouth of an estuary, a *wave of derivation* is detached, which passes up the river. The duration of the tide in the river is the same as that of the ocean tide which caused it, but this is the only point which the two have in common. The tidal wave in the river is essentially a wave of translation, or rather a series of such waves, for Mons. Comoy says:—

"When the sea, lifted by its undulatory movement, begins to exceed in height the level of low water in the river, the water projected into the river produces a first wave which travels up the stream with a velocity proportional to the low-water depth. After this first introduction of the water of the sea, and continually during the whole of the flood tide, the sea, always higher than the water in the river, projects into it new masses of water which give rise to new waves of translation. All of these waves are propagated, like the first, in virtue of the law of their regimen, while new waves are constantly forming at the mouth of the river. At any given moment, therefore, of the flood tide, we find between the embouchure and the first of the flood, a continuous series of waves of translation occupying the entire length of the tide, and forming by their aggregation the forward slope of the tidal wave of the river, at that moment."

For the velocity of propagation of the crest of the tidal wave in the river, we have

$$V=\sqrt{g\,H}-U$$

the same as for the ocean wave less U, or the down-stream velocity of the river.

As the depth of water in the river diminishes, $\sqrt{g\,H}$ diminishes also. When for any elementary wave it becomes = U, that wave ceases to exist.

The distance traveled by a molecule of the water of the flood-tide is given by the formula

$$l=T\,V\frac{v}{V-v}$$

in which l = distance; T = time in seconds of duration of flood; V = mean velocity of propagation of flood-tide during the time T; v = mean velocity of flood current over the distance l.

Observed velocities show that the ratio $\frac{v}{V-v}$ is rarely less than $\frac{1}{3}$, while in some rivers of shallow depth it becomes $\frac{1}{2}$.

This formula has been verified by observing the distance to which the salting of the river water by that of the sea extends in some of the French rivers.

In the chapters devoted to the mascaret, Mons. Comoy goes into great detail respecting the cause and theory of this phenomenon. We will not follow him through this interesting investigation, further than to state the conditions which he finds to exist whenever the mascaret is produced.

The principal condition necessary to the formation of the mascaret, is as follows: during any given period of the flood-tide, a certain quantity of tidal water enters the embouchure, represented by Svt, in which t = any given time, during which the tide at the embouchure rises through the distance C; v = mean velocity of flood-tide at the embouchure, during the time t; S = mean cross-section of the embouchure of river during the time t. This quantity of water after entering the river occupies a volume represented by DLA; in which D = mean distance between the embouchure and the first of the flood: or the distance traveled up the river by the tidal wave, during the time t; L = mean width of river over the distance D; A = mean rise of the tide during the time t, over the distance D.

These two volumes are necessarily equal, for they represent the same quantity of water in two different positions, so we have

$$Svt=DLA.$$

If a mascaret is produced, it will always be found that in the above relation, A > C. C, it will be observed, forms part of S.

In other words, it is found, that when the mascaret is produced, the height to which the tide rises in the river during the time t, is greater than that of the rise of tide at the embouchure, during the same time. Moreover, it is found, that, leaving out exceptional cases, the mascar-

et only exists in rivers of which the shoals are formed inside of the mouth of the river.

In regard to the improvement of tidal rivers, Mons. Comoy confines himself to the consideration of the effects upon the regimen of rivers, by deepening their shallow places, without discussing the means by which such deepening may be effected. He makes the very true remark that the regimen of rivers differs so much in different cases, that we may say with justice that there are as many particular regimens as there are rivers. Some features, however, similar rivers possess, in common, and these are ably studied.

Two very broad classes may be made of tidal rivers; those of which the bar is formed *within* the mouth, and those of which the bar is formed *outside* of the mouth. (In this latter case only, is the shallow portion termed *bar* in English.)

Mons. Comoy states, that in all rivers where the bar is formed outside, or to seaward of the embouchure, there is a marked narrowing of the mouth, the river being much wider for a certain distance above, than it is at the mouth, or embouchure itself. This narrow part is consequently deep, and the bar is formed (*perhaps not exclusively*) by the scourings of the narrow portion, where the velocity is great, being deposited outside the mouth.

In rivers where the bar is formed inside the mouth (or where, as we should say, there is no bar) the embouchure is on the contrary wider than the rest of the river. It is in such rivers that the mascaret is most prone to exhibit itself.

In order to obtain actual data regarding the regimen of tidal rivers, the *Administration des Ponts et Chaussées*, authorized observations to be made for Mons. Comoy, of several of the most important tidal rivers of France. These observations were made simultaneously during the spring-tide of Sept. 19th 1876 and the neap-tide of the 26th of the same month and year. "Instantaneous curves" were thus obtained of great value, which are given in the atlas accompanying the work.

In the foregoing we have done but little more than, as it were, rapidly turn over the leaves of an exceedingly exhaustive and practical treatise upon tides and tidal rivers. Many valuable chapters have been passed over without notice, and over none have we dwelt long. Enough however, it is hoped, has been said to give some slight idea of the character and scope of the volume, and to commend it to the careful study of those scientists and engineers who are interested in the subjects upon which it treats.

TELPHERAGE.*

From "Iron."

The following short summary of the problem of the transmission of power by means of electricity may interest those who have not studied the subject. There are three steps in this transmission—1st, we convert mechanical power into electricity by means of a dynamo; in doing so we incur a loss of from 10 to 20 per cent.; 2d, this electricity, in flowing along a conductor, generates heat, representing a further loss, analogous to that resulting from friction in mechanical gearing. This loss, depending on the distance of transmission, the size of the conductor, and the electromotive force employed, is easily computed. 3d, we re-convert the electricity into mechanical power by means of an inverted dynamo, which we term an electric motor. With motors in which large weights of iron and copper are employed, the loss in re-conversion need not exceed 20 per cent., but with light motors, weighing from 70 lb. to 100 lb., per horse-power, such as we must employ in the locomotives, I could not undertake with certainty at this moment to effect the reconversion without a waste of one-half. The effect of all these sources of loss is, that at the stationary engine I must exert about 3 horse-power for every single horse-power which is employed usefully on the line. I look forward con-

* Abstract of Prof. Fleeming Jenkin's paper before the Society of Arts.

fidently to the time when 2 horse power at the engine will be sufficient to give 1 horse power to the motor. To put these conclusions in a more scientific form, I may assume the efficiency of my dynamo as 80 per cent., that of my small motor as 50 per cent.* The waste by heat expressed as horse-power is equal to $\frac{C^2R}{746}$ where C is the current in amperes, and R the resistance in ohms. The horse-power represented by the current is equal to $\frac{EC}{746}$ where E is the electromotive force in volts, and C the current in amperes. It follows from the last expression that I may increase the horse-power in three ways, by increasing either E or C, or both. If I increase E, leaving C the same, I do not increase the loss during transmission along the line, no matter what horse-power the given line may transmit. A practical limit is set to the application of this law by the difficulty met with in dealing with electromotive forces above 2,000 volts. Marcel Deprez, taking advantage of this law—first pointed out by Sir William Thomson—has transmitted seven or eight horse-power over seven or eight miles, through an ordinary telegraph wire, and he obtained a useful duty of 63 per cent., taking into account all the three sources of loss which I have enumerated. With small motors I cannot yet promise a result so good as this, and I merely mention it to let you understand that, in speaking of 3 horse-power for one at the locomotive, I am leaving a very ample margin. Quitting generalities, I will give some details as to the electrical and other conditions necessary, in two examples, for what may be considered as typical telpher lines.

First line.—Length, five miles. Length of circuit, out and in, ten miles. Twenty-five trains running at once, spaced one-fifth of a mile apart; speed, four miles per hour. Let each require 1 horse-power on the average; let the motor take on the average two amperes of electric current; let the electromotive force near the stationary engine be 840 volts; the electromotive force at the end of five miles will be about 746 volts.* The total current entering the line will be fifty amperes at the near end of the line. Fifty amperes and 840 volts represent 56.5 horse-power; of this, 6.5 horse-power will be wasted in heating the line; the remaining fifty horse-power will do work in the motors equivalent to 25 horse-power. In order to give this current of fifty amperes with 840 volts the stationary engine will require to exert $\frac{1}{.8^0} \times 56.6$ horse-power, or roughly, 70 indicated horse-power, or somewhat less than three times the useful horse-power. Let us now examine the economical results to be obtained from such a line as this. Mr. Dowson, in an interesting comparison between the cost of horse-power obtained from coal and gas, reckoned the cost per horse-power for a 100 horse-power engine, at the rate of £3 6s. 9d. per annum, to include wages, coal, oil, and depreciation. Mr. Dowson would naturally be led to put the cost of steam power obtained from coal rather high than low. I will, however, adopt a very much higher figure, and assume that the power may cost as much as £6 10s. per horse-power per annum; this gives £455 as the cost of the 70 horse-power required for my telpher line. Let the twenty-five trains each convey a useful load of 15 cwt. In a day of eight hours the line will have conveyed a traffic which we may express as 600 ton-miles, *i. e.*, it will be equivalent to 600 tons conveyed one mile, or 60 tons on each line conveyed from end to end daily. If we count 300 working days in the year, the sum of £455 gives £1 10s. 4d. per diem, and the 600th part of this is about 0.604 of a penny, as the cost of the power required to carry a ton one mile. In Great Britain we ought easily to be able to reduce this below a half-penny per ton per mile, which proves that the apparent great waste, even of two-thirds of the power in transmission, does not involve prohibitory expense. In calculating the whole cost of transport, we must further take into consideration the cost of the installation. Taking the spans at 70 feet, I estimate this cost as follows:

* The last motor tested, weighing 117 lb., made by Mr. Reckenzaun, gave 1.72 horse-power with 54 per cent. efficiency.

* It has been suggested that this fall of potential would be inconvenient for electric lighting, it would be too great; but in Telpherage it would simply cause a small decrease in the speed of the train at the far end, and as all trains would be equally affected, none would overtake the other, and no inconvenience whatever would arise.

Line £500 per mile	£2,500
Engine, boiler, and shed, at £20 per indicated horse-power	1,400
Dynamo and fittings	1,000
Twenty-five trains	2,500
Contingencies	600
Total cost	£8,000

Allowing 12½ per cent. for interest and depreciation, this represents an annual cost of £1,000. Allowing £100 as the salary of an electrician or young engineer, and adding £455, the cost of the power, this gives a total annual expenditure of £1,555 for a daily duty of 600 ton-miles. If we continue to assume the year as containing 300 working days, the total cost of conveying one ton one mile will be found equal to 2.07d. If goods are to be transmitted for long distances, the same calculation applies. We should simply have stations ten miles apart, working lines five miles long on each side of them. This, then, is the practical outcome of the general principles stated at the beginning of this paper. We may expect with great confidence that it will pay investors to convey goods for any distance at the rate of 2d. per ton per mile, by the agency of the suspended telpher line.

Second Line.—Matters are somewhat modified when the traffic is smaller. Making similar calculations for a line one mile long instead of five, with only four trains running at once, we might employ an electromotive force as low as 100 volts; the loss by heating would be insignificant; we should require about 12 horse-power; the work done in eight hours would be 96 ton-miles. I estimate the cost of installation at £1,600, and the annual cost of working £344, without the annual salary of an electrician. This corresponds to 2.875d., or less than 3d. per ton per mile. One very important feature in respect to the cost of telpher lines is the fact that the larger part of that cost is due to plant, such as locomotives, trains and dynamos. This plant can be increased in proportion to the work required; thus there is a very moderate increase of cost in the rate per ton per mile for a small traffic as compared with a large one, and, on the other hand, a line laid down for a small traffic will accommodate a much larger traffic with no fresh outlay on the line itself.

There are numerous minor electrical problems involved, but time does not permit me to enter into the consideration of these to-night. It will be sufficient for electricians when I say that I se mey way to governing, blocking and breaking the trains, without ever interrupting the current used to work the motor, except between the line and rolling wheels. We already know that the interruption at this point, although accompanied by a spark, does no injury whatever. I have often been asked whether the frequent reversals involved in the cross-over system do not tend either to injure the dynamo or the motor. I made special experiments on this very point lately with a compound wound Crompton dynamo and Mr. Reckenzaun's motor with thirty-six coils. I was unable at the commutator of the motor to detect the smallest change in the motion due to the most rapid reversal. At the dynamo commutator I could just see when the reversal occurred, but there was no change of a character to cause the smallest alarm. At the same time I may state that, when from any cause reversals may be thought undesirable, we are in possession of apparatus which we call "step overs," which, without diminishing the simplicity of the permanent way, enable us to send a continuous and unreversed current. These and similar electrical questions, such as the performance of Messrs. Ayrton & Perry's excellent motors, might possibly have had greater interest for electricians than some of the mechanical details discussed to-night; but I have felt that the main point to establish, in bringing this invention before the public, is that we have in telpher lines a means of conveying goods in an economical manner, by lines, locomotives, trucks, dynamos and motors, which have undergone their preliminary trials with success, and can be at once applied to the more searching test of performing work for the public. If I have established this fact, I think you will have no difficulty in believing that the subsidiary electrical problems have been, or will be, readily solved. I hope that at a future period these will be brought before you in detail on many occasions by many men. In conclusion, I will enumerate some of the uses to which telpher lines may be put. They will convey goods, such as grain, coals and all kinds of minerals, gravel, sand, meat, fish,

salt, manure, fruit, vegetables; in fact, all goods which can be divided conveniently into parcels of two or three hundredweight. If it were necessary, I should feel no hesitation in designing lines to carry weights of 5 or 6 cwt. in each truck. The lines will carry even larger weights, when these, like planks or poles, can be carried by suspension from several coupled trucks. The lines admit of steep inclines; they also admit of very sharp curves. Mere way leaves are required for their establishment, since they do not interfere with the agricultural use of the ground. They could be established instead of piers, leading out to sea, where they would load and unload ships. With special designs, they could even take goods from the hold of a ship and deliver them into any floor of a warehouse miles away. When established in countries where no road exists, the line could bring up its own materials, as a railway does. Moreover, wherever these lines are established, they will be so many sources of power, which can be tapped at any point, for the execution of work by the wayside. Circular saws, or agricultural implements, could be driven by wires connected with the line, and this without stopping the traffic on the line itself. In fine, while I do not believe that the suspended telpher lines will ever compete successfully with railways, where the traffic is sufficient to pay a dividend on a large capital, I do believe that telpher lines will find a very extended use as feeders to railways in old countries, and as the cheapest mode of transport in new countries. In presenting this view to you, I rest my argument mainly on the cost of different modes of transport, which may, I believe, be stated approximately as follows: Railway, 1d. per ton per mile; cartage, 1s. per ton per mile; telpher lines 2d. per ton per mile; and let it be remembered that, in taking the cost of cartage at 1s. per mile, the first cost and maintenance of the road is left wholly out of account; whereas in my calculations for the telpher line allowance has been made both for establishment and maintenance.

THE NILE DELTA.

NOTES OF A JOURNEY THROUGH THE N.E. FORTION OF THE DELTA OF THE NILE IN APRIL '81.

By WILLIAM ANDERSON, M. Inst. C. E.

From Papers of the Institution of Civil Engineers.

HAVING been commissioned to ascertain why extensive estates belonging to an English Company were wholly unproductive, and indeed incapable of earning the Government land-tax, it is thought that the observations then made may be useful to all interested in the true welfare of Egypt. The district visited was that portion of the Delta lying between Mansoura, a large town on the Damietta Nile, and Lake Burlos, a lagoon connected with the Mediterranean, and thus quite outside the usual track of visitors. The inquiries and investigations were of a definite and practical character, and have produced a profound impression as to the state of the administration, and as to the manner in which the resources of the country are destroyed at their very origin.

The welfare of the agricultural industry, the only source of wealth in Egypt, depends mainly on the efficiency of the irrigation, the drainage, and the means of communication. It soon became apparent, that the estates visited, and many thousands of acres of land around them, were suffering from startling defects in all the three conditions on which prosperity depends.

First, as regards Irrigation. The portion of the Delta inspected, and, indeed, the whole Delta, is intersected by innumerable canals, some used for drainage and some for irrigation. These have been dug at various times, for the most part without reference to any general plan. They are practically devoid of proper locks or sluices for regulating the flow of the water, the method adopted

being to throw banks of earth across the canals, leaving gaps of sufficient width to control the water, these gaps being regulated by stakes, planks, or lumps of mud wrapped in rice straw. Owing to the caprice, corruption or laziness of the officials, the water in the irrigation canals is often too low to command the land, even when the Nile is high enough for the purpose, and at other times the water is allowed to pass so freely as to overflow the banks and submerge large tracts of country. In summer time, between the months of May and of August, during low Nile, many of the canals, for want of proper cleaning and maintaining at the required depth, become quite dry, and thousands of acres of summer crops, such as cotton and rice, perish for want of water. In the district visited, two-thirds of the crop was, last summer, completely lost from this cause. But even when the canals are properly cleaned out, the water is still below the land-level, and every drop required for irrigation has to be raised by imperfect sakiehs, and small steam-pumps. These may be seen dotting the landscape in all directions, and represent an incredible amount of human and animal labor diverted from the more legitimate work of tilling the soil. The cost of this wasteful mode of raising water is at least ten times greater than that for which the same work could be done by large Government pumping stations, or by a better management of arterial canals. This state of things is peculiarly distressing just at this time, when the number of bullocks has been greatly reduced by a formidable epidemic, so much so, that it will be impossible, this season, to cultivate even so much of the land as is capable of being worked.

In the next place, the drainage-canals have been much neglected, especially at their lower ends or outfalls. They all require widening and deepening, so as to carry the drainage waters into the sandy wastes skirting the Mediterranean. The fall of the country is rapid, so that there is no difficulty in securing very efficient drainage. The neglect of the Works Department to attend to this important part of its duties, injures not only the lands depending upon the public canals, but also estates such as the ones visited, which, lying on the edges of the waste, had their own private drainage-canals; for it is in vain that these are kept in proper order, when the water standing on the adjoining lands percolates through the soil, and rises in the better drained portions, bringing up with it the deleterious salts with which the whole subsoil of the Delta is more or less impregnated, so much so, that this salt may be seen lying like snow over vast areas of land. It is absolutely essential to the fertility of the land that the drainage-canals should be constantly kept clear, and that there should be an unfailing supply of irrigation-water all the year round, because it is only by keeping a regular wash of water from the surface downwards that the deleterious salts can be prevented from rising; but when this is done, the fertility of the soil is marvelous, and the richness of the crops is, for the most part, assured. Adjoining one of the estates was a property of 1,400 acres which had been abandoned by its owner for no other reason than that the Government drainage-canal was choked with mud and aquatic plants. The Government had seized the land for taxes; it had twice put up the property for sale without finding a purchaser, and, of course, had lost the revenue from the lands. A single year's taxes would have sufficed to clean out the canal. This is a particular case, respecting which definite information was obtained, but many thousands of acres of other lands were in a similar predicament, as, for example, of the 100,000 acres belonging to the village of Massara, only 12,000 acres are capable of cultivation, on account of the neglect of the drainage and want of irrigation-water.

Lastly, mismanagement has practically destroyed the means of communication. The canals, if properly maintained, would be wide and deep enough to carry cargo-boats as large as those in use in England. These arteries of communication intersect the country in all directions, and yet it costs 26*s.* a ton to bring the produce from the estates visited, a distance of only 33 miles, to the Damietta Nile, and even that can only be achieved during the dry season, because rain makes the towing and bridle-paths impracticable. This state of things could be remedied at a most insignificant cost.

The impediments to cheap communication in a country so highly favored with

what may be termed natural roads are the following:

When the canals are cleaned, the mud is thrown up on to the towing-path, and allowed to harden in the sun, without the least attempt to level it or remove it altogether from the path. The consequence is, that the animals using the bank of the canal tread a bridle-path on a slope. This at all times is difficult foothold, and in wet weather, when the mud gets slippery, becomes dangerous, and even impracticable. Trees are allowed to grow between the towing-paths and the canals, so that in many places towing is impossible, and from the opposite bank trees overhang, so that the towing masts cannot pass. Naturally all these obstructions should be removed. The banks thrown across the canals for the purpose of regulating the flow of water render trans-shipment necessary, and in this way, in the 33 miles of canal specially visited, there were no less than four dams of weirs, necessitating four trans-shipments. The difference of level between the two sides of the dams is never more than a few inches. It would be very easy and inexpensive, in the tenacious soil of the Delta, to construct wooden dams, with lock gates and sluices, which would allow of the barges being locked through, and dues, which would amply cover all expenses, would be cheerfully paid, as they are in other countries. Again, no control is exercised over the cutting of the canal-banks for irrigation or drainage-branches. The people cut through the banks, and neglect to make them good again, thus intersecting the roads with innumerable pitfalls, which render travelling in the daytime fatiguing, and at night impossible. Surely a very moderate amount of supervision would put an end to this nuisance.

Egypt depends for its prosperity on its agricultural produce; but the competition of the whole world, the immensely improved ways of communication in America, in India, in Russia, have had the effect of reducing the price of all the products which Egypt can send into the market, more especially that of cereals, and the price is never likely to rise permanently again. How is it possible that those portions of Egypt which lie at a distance from Alexandria can hope to export their produce when the means of communication are so defective? The inevitable result will be that the country must relapse into a state where just enough is raised to feed a miserable population, and no taxes will be paid, because, there being no sale of produce, there can be no money.

Whatever neglect there may be in the maintenance of the canal-system, there is no slackness in collecting the land-tax, and defalcation is quickly followed by seizure and forced sales, entailing ruin, misery and discontent among the agricultural population.

The conduct of the Administration is inexplicable, because it would entail very little more than the present expenditure to keep the canals in proper order, while it would improve the revenue by making a greater breadth of land cultivatable, and would restore to the land the human and animal labor now lost in raising water in a wasteful manner, and in overcoming the difficulties of transport.

The people do not object to forced labor on their own canals. They have sense enough to know that by no other means can the life-giving streams be maintained, and it is possible so to distribute the work, and so to select the season for doing it, that the task would not press seriously on the people. At any rate, it would be an infinitely smaller infliction than that which now exists as the consequence of neglect and mismanagement, by which the poor struggling peasants see the fruits of their labor disappear year by year, for want of irrigation and of drainage, or, if the harvest be haply secured, rendered valueless by the cost of carriage.

Many distinguished men have, from time to time, been sent to Egypt to set right its finances. Many ingenious schemes have been proposed, but it is doubtful if any who have attempted the regeneration of the finances of the country have turned their attention to the root of the matter. Measures should be adopted to place the cultivatable portion of Egypt in a condition to produce the wealth latent in its lands, in its river, and in its laborious peasantry, and further to provide cheap and expeditious ways of communications, which can alone enable the produce of the lands to be converted into the gold which the financiers thirst for. It would be a

worthy task for England to step in and remove the terrible drawbacks under which the fellaheen suffer. In her Indian engineers and their subordinates she has an admirable and experienced staff of administrators. The same men, the same system, and the same honesty of purpose introduced into Egypt would make a garden of a country which is now almost a wilderness.

A step in the right direction has already been made in the appointment of Colonel Scott Moncrieff, R. E, to the post of Under Secretary of State for Public Works. He has earned a high reputation in India, and will assuredly, if properly supported, do much towards the regeneration of the country entrusted to his care.

Unfortunately, long neglect and mismanagement have rendered large expenditure necessary, while that very neglect and mismanagement have crippled the revenue.

Colonel Moncrieff, has already been ordered to retrench in every possible way, even when it is certain that expenditure such as he contemplates will yield enormous return. He is perfectly alive to the importance of the reforms alluded to. In two "Notes" addressed to the Financial Advisers of the Government, one dated February 13th, 1884, on the communications throughout Egypt, and the other, dated March 4th, on the Public Works which he deems essential to the good administration of the country, he points out in terse and vigorous language the real barriers interposed to prosperity, in the absurd and oppressive taxes on navigation, the impediments offered to it by bridges over the main channels, the defects of drainage and irrigation, and the total absence of roads.

It is satisfactory to be able to state that recently in Egypt one great concession was announced, namely, the river and canals were set free for steam-navigation, a privilege till then reserved to the Government. It cannot be doubted but that this has been one of the results of the improved administration of the Public Works Department, and it is to be hoped that many other reforms will soon follow.

The uncertainty which surrounds the intentions of the British Government is stopping all public and private enterprise, and the poor farming population, that is to say, the bulk of the people of Egypt, is sinking deeper and deeper into a hopeless condition of poverty and debt. This is not a mere statement of opinion; on the contrary, the existing political situation had to be taken into consideration. The canals and drains on the estates visited had long been neglected. An expenditure of £1,500 in cleaning out was absolutely necessary, if advantage was to be derived from the expected improved irrigation and drainage service; and could the company have felt sure of the intentions of the British Government to continue steadily in the course they have entered upon, the work would have been commenced at once. The probabilities are, indeed, in favor of a prolonged occupation, because a great number of British officials have entered the Public Works Department, replacing some able and experienced men, such as Rousseau Pacha and his chief engineer, and it is hardly credible that the British Government would make such appointments if their sway was to be only of a temporary nature. But still the Government assurance is wanting, and it is impossible to advise expenditure of capital which would be fruitless in the event of the withdrawal of the British, for in that case the Government of Egypt would relapse into its original state of inefficiency.

In 1883 the Ministry of Public Works in Shereef Pacha's Government published a most able paper entitled "Exposé du Ministère des Travaux Publics sur les Irrigations d'Egypte, et examen d'une proposition y relative."* It is signed by Ali Pacha Moubarick, then Minister of Public Works, and by Rousseau Pacha, their Director-General. These gentlemen are both men of long experience in the department over which they presided, and Rousseau Pacha, especially, is an engineer of great and varied information. The "Exposé" treats first of the general systems of irrigation and drainage in vogue in Upper and Lower Egypt, discusses the capacity of the Nile at low water, and demonstrates that its volume is barely sufficient for the wants of the existing summer cultivation, and those of navigation. The long-standing question

* An abstract of this will appear in the Minutes of Proceedings, vol. lxxvii.

of the Barrage is carefully considered, and the conclusion is reached, that even supposing by an expenditure of very large sums of money the Barrage could be repaired, or, as they point out, more properly rebuilt, and the banks of the arterial canals raised to a proper height, to convey the waters pent up behind it, Government might well recoil before the enormous risk of so powerfully altering the regimen of a river such as the Nile, and incurring the consequences of injury to a wide extent of productive land brought on by the infiltrations which would be sure to supervene. In addition, they draw attention to the enormous cost of keeping the arterial canals clear, and the absolute necessity of employing for that purpose forced labor in its most odious form, that is, at a distance from the homes of the laborers, and employed on works in which they have no direct interest. The height to which the banks of the arterial canals have risen from centuries of cleaning, renders the labor more and more severe each successive year, and more and more land is being covered by the mud excavated, and by that means rendered valueless. The Rayah of Khatatbeh, in the western margin of the delta, for example, which used to bring the waters of the main Nile from above the Barrage to the Khatatbeh canal, which irrigates the Province of Behera, though only 26 miles long, required annually the labor of 20,000 men for fifty days to clean it. The bare cost of feeding this army used to amount to £12,000, and if to this the labor lost to the farms and valued at only 6d. per head per day be added, there was a further loss to the country of £25,000, making £37,000 per annum for 26 miles of one canal!

In addition, the mud deposited in the arterial canals is the fertilizing essence of the Nile water, and should be deposited on the lands irrigated, and not in the canals. Too much importance cannot be attached to this point. The canals should be as short as possible, and then their currents can be so regulated that the least possible deposit shall take place.

On all these grounds the Exposé strongly advocates the use of powerful steam-pumps, under Government control, in preference to attempting to patch up the Barrage, or make use of long arterial canals. The system would abolish the most oppressive kind of forced labor, would avoid all risk of infiltration, would assure abundant water at all times at a height to command the land irrespective of the level of the Nile, and last, but not least, would save the Government for some years to come, in a great measure, from the very heavy annual outlay in cleaning canals used for the summer water, which now have to be maintained at a great depth, but which, if pumping be resorted to, may be allowed to silt up permanently to a very considerable extent. The Government of Egypt, before the rebellion, had already acted on the principles laid down in the Exposé, and had established a powerful pumping-station at Khatatbeh, and increased the existing one at Atfeh, so as to save the expense and oppression involved in cleaning the Rayah of Khatatbeh. The Government of Shereef Pacha took up the good work, and all but made a contract with an English company to utilize its powerful pumps at Cherbine, which would have supplied the district visited, and invited tenders for the erection and working of another similar station for the Mansourah district. Unfortunately the troubles in the Soudan broke out, Nubar Pacha replaced Shereef, and the arrangements made by the Government of the latter were in a great measure set aside. Colonel Scott Moncrieff (who was appointed about this time) was forced to reduce his budget at the expense of many salutary reforms, and unfortunately with respect to the water supply of the district, which forms the subject of these remarks. At first Colonel Scott Moncrieff seemed favorable to the views advocated in the "Exposé" of the late ministry; but after the Soudan troubles, driven, perhaps, by pecuniary necessity, he unfortunately determined to make an attempt to raise the waters of the Nile by means of the Barrage, and that work is now being strengthened in a manner which he expects will enable it to perform the office for which it was designed. Most experienced engineers, native and foreign, believe that the attempt will end in failure. Mr. B. Baker's graphic description of the work* is of a very discouraging kind, especially as it

* Minutes of Proceedings Inst. C. E., 1879-80, part ii.

is known that when the Barrage was being examined with the view to converting it into a railway bridge, a diver was actually able to make his way right under and across a portion of it! Colonel Moncrieff is very frank about his attempt, and is quite prepared for a failure; but a failure may prove very disastrous indeed, because, as Rosseau Pacha points out in the Exposé, the Barrage now serves an invaluable purpose in regulating the amount of water passing down the Damietta Nile. Mr. Baker mentions the tendency of the river to follow the shorter, or Rosetta branch, hence, if any serious failure of the Barrage were to occur, the lands depending on the Damietta Nile would suffer severely. Colonel Scott Moncrieff and his assistant have been but a short time in the country; they have not yet seen the Nile through all its phases. It would surely have been more prudent to postpone the dangerous experiment of dealing with the Barrage to a future time.

Although on the present occasion but a small portion of the country was visited, yet at other times the author has traversed many miles of the canal-system both in Upper and Lower Egypt, and everywhere the same neglect of the vital interests of agriculture exist. Nothing but a strong Government, served by capable and honest officers, will be able to achieve the reforms which will place the irrigation, drainage and communications of Egypt on a par with those of the other civilized countries with which it has to compete. The grand schemes which have from time to time been broached for the construction of regulating reservoirs on the Upper Nile are, as Rousseau Pacha points out, works requiring long years and large sums of money. But the immediate necessities of the cultivatable lands can be satisfied with but moderate outlay, and the returns will be immediate and abundant if the expenditure is controlled by capable and honest administrators; but the work must be done at once if the revenues of Egypt are to be maintained. The Ministry of Public Works has declared emphatically that "the country cannot wait."

TEMPERATURE OF THE SUN.

By F. GILMAN.

Written for Van Nostrand's Engineering Magazine.

In the last July number of this Magazine there appeared an article written by Prof. DeVolson Wood, in which he criticised Capt. J. Ericsson's method of determining the sun's temperature; and made many statements which are undoubtedly erroneous, as we will proceed to show.

He says: "The law that the intensity of heat diminishes as the square of the distance from the radiant increases, is applicable only to the case where the radiant body is considered as a mere point, and even in those cases the law does not give the temperature of the incandescent body. It is evident that Prof. Wood refers to a mathematical point, which has position but not magnitude, and assumes that a material body can occupy such a point, and, to cap the climax, tells us that the temperature of this point, or body, must be infinite! As well might he say that the law that the intensity of gravitation diminishes inversely as the square of the distance is applicable only to a point, or (to speak more rationally) to a material point, or particle of a body. But it has been proved that the same law of gravitation prevails in a spherical homogeneous mass of any dimensions, when the distance is measured from the center; and it is also applicable in the case of heat radiating from a spherical body, such as the sun; for gravitation, light, and radiant heat, conform to the same law. Prof. Tait, in his *Treatise on Heat*, declares that this law is necessarily true of any form of energy which spreads rectilinearly in all directions.

Prof. Wood says that the law does not give the temperature of the incandescent body. No, to be sure it does not; but it gives the temperature of the medium in the vicinity of the body, and the temper-

ature of the body cannot be lower than this, but must be higher, unless the emissive power is equal to unity.

Capt. Ericsson has proved that the temperature of the medium in the vicinity of the sun is at least 1303640° Fahr. The temperature of the sun itself must then be fully equal to this, and is probably much higher. Prof. Wood says: "Experiment shows that the quantity of heat emanated from an incandescent body increases more rapidly than the temperature." This statement is incorrect. The quantity of heat lost by an incandescent body, in a given time, does not depend simply on its temperature, but rather on the difference between its temperature and that of the surrounding medium.

Prof. Wood gives the law deduced by Petit and Dulong in the following equation: $2=1.146 f a^t$.

We suppose there were some typographical errors in the explanation of symbols that follows, and therefore we will give what appears to have been the writer's meaning, in order, if possible, to get a little sense out of the formula. 2 is the quantity of heat per square centimeter emitted per minute, f the emissive power of the surface, its superior limit being unity; $a=1.0077$ while t is the temperature in degrees Centigrade of the hot body. It will be noticed that nothing is said about the temperature of the medium in which the body is placed, but let us suppose that it is constantly kept at $o°$ C., as this supposition is the best that can be made consistent with the formula. Even in this case, however, it is seen to be incorrect; for making $t=o$ we have

$$2=1.146 f.$$

That is, according this formula, when the body has cooled down to the temperature of the medium it is still losing heat, which is impossible; therefore the formula is false, or at least incomplete. The following formula given by Piclet in his *Traite de la Chaleur*, p. 373, vol. I., contains the proper expression of Dulong and Petit's law:

$$R=(124.72)\,K a^{\theta}(a^{t}-1)$$

R represents the quantity of heat emitted by radiation per square meter per hour, K a number which depends on the nature of the surface, a the constant 1.0077, θ the temperature of the medium, t the excess of the temperature of the hot body above that of the medium.

Prof. Wood, who expects such great things from this law of Dulong (which he has incorrectly given), seems to think that it holds true for any excess of temperature, however great; and would even, with Pouillet, apply it to determine the sun's temperature. In reply to this we quote from two authorities to show that Dulong's formula is only empirical, and that it ceases to be true when the excess of temperature is over 200° C.

"*On voit donc d'abord que la formule de Dulong est empirique, puisqu'elle englobe dans une expression commune des elements qui sont differents, et ensuite, qu'elle doit cesser d'être exacte a des temperatures elevees.*"

Jamin's Physique, Vol. ii *p.* 314.

De la Provostaye and Desains have verified the consequences of this formula from o° C., to 200° C., but they state that above the latter temperature it is no longer applicable.

Tait's Heat, p 279.

Hence appears the absurdity of applying such a formula as this to determine the amount of heat radiated (or conversely when the amount radiated is known to find the temperature) from a body like the sun, in which the excess of temperature is so enormous.

The principle which Capt. Ericsson applies, however, to determine the sun's temperature is perfectly general, and holds for any excess of temperature. It is susceptible of mathematical demonstration; and Capt. Ericsson has given in his sun motor an experimental demonstration of it, which is unanswerable.

It seems as if some idea might be obtained in regard to the sun's temperature, by calculating what the temperatures at the earth would be, supposing it to advance by successive stages toward the sun. In order to know how much heat is communicated at present, we must conceive what would be the result if the sun were removed from the universe; or if its heat were entirely extinguished. In that case it is probable that the temperature here would not be far from that of the absolute zero, 460° below zero, Fahr.; and therefore the earth must at present receive from the sun an increment of nearly 400° Fahr. But we will take a much

smaller estimate, and suppose that it is only 100°, and then proceed to calculate what would be the temperature at the earth's surface, were it advanced half-way to the sun's center, or at a distance of 46,000,000 miles. According to the principle above stated, the earth would then receive four times the quantity of heat that it does at present; and assuming (which we may safely do in a question of this kind) that equal increments of heat correspond to equal increments of temperature, the temperature at the earth would be $4\times100°=400°$ reckoned from the absolute zero, or 60° below zero, Fahr., I have assumed an initial temperature far below that known to exist, in order to show to what a high temperature even that hypothesis will lead us, when we reach the sun.

Next, let the earth advance until it is 23,000,000 miles from the sun's center. The temperature will be $4\times400°=1,600°$. Continuing this process we shall find that when the earth is 718,750 miles from the sun's center, or 275,750 miles from its surface, the temperature will be 1,798,-400° Fahr.

THE STUDY OF IRON AND STEEL.

By J. C. BAYLES, New York City.

Transactions of the American Institute of Mining Engineers.

Gentlemen of the American Institute of Mining Engineers; Ladies and Gentlemen: The propriety of imitating in everything, so far as I am able, the worthy example of the distinguished gentlemen who have dignified the honorable office of President of the American Institute of Mining Engineers, imposes upon me the agreeable duty of delivering an address from the chair. The prominence which has been accorded in the programme of this meeting to the discussion of subjects connected with the study of metals, especially iron and steel, has naturally given direction to my thought.

It seems to be characteristic of invention and investigation that they conform to no law of regular and uniform development. From the measurably firm ground of accepted truth and verified experience, the work of original investigators is projected into the void of the unknown; and so rapid and important are the accretions of fact around such slender spars of well-directed speculation, that it seems for a time as if we might go on extending and building them up until the void was fully and safely bridged. But such a line of investigation is like a cantilever with a pier at one end and nothing at the other. The limitations, not only of knowledge but of speculation, become evident as we load hypotheses upon the unsupported end of our structure; and to make our work of value, we must find a solid basis somewhere else, build thereon a pier, and project therefrom a second cantilever. When these meet and are securely united, we have spanned one of the spaces between facts learned by observation and experience, and can safely pass over to a point from which new speculations and verifications may serve as a basis for further progress.

We are impressed with the appropriateness of this figure when we examine the steps by which we have gained what little knowledge we already possess of the composition and properties of iron and steel. That we know as much as we do concerning them is surprising, when we reflect that among our membership are many whose lives almost include the period in which these materials have been intelligently and systematically studied.

The progress of chemical science applicable to iron and steel analysis, naturally invites attention first. Before quantitative analysis was attempted, certain of the crude reactions of qualitative analysis were recognized. Paracelsus, the marvelous charlatan who lived from 1493 to 1541, knew of some of these; and Boyle, an earnest worker in this field, records several in his "Essay on the Usefulness of Experimental Philosophy," published in 1671. Marggraf, who lived from 1709 to 1783, is the first chemist who is credited with analyses of minerals. Thomson, in his history of chemistry (London, 1831), says

of Marggraf's work: "His attempts were rude, but their importance was soon perceived by other chemists, particularly by Bergman (1735 to 1784) and Scheele (1742 to 1786), whose industry and address brought the art to considerable perfection." Bergman, whose *De Analysi Ferri* was published in 1770, has left a very interesting record of his experimental work, which contributed in a material degree to advance the knowledge of the difference between iron and steel. He employed his pupils to collect specimens of iron from the different Swedish forges, and all of these specimens, to the number of eighty-nine, he subjected to a chemical examination by dissolving them in dilute sulphuric acid. He measured the volume of hydrogen gas which he obtained by dissolving a grain weight of each, and noted also the quantity and nature of the undissolved residue. The general result of the whole investigation was that pure malleable iron yielded most hydrogen gas, steel less, and cast iron least of all. The amount of Bergman's knowledge and the value of his methods may be judged from a table of percentages which he has left us, giving the composition of cast iron, steel and wrought iron. This table shows the following results:

	Cast iron.	Steel.	Wrought iron.
Inflammable air.	40.	48.	50.
Plumbago	2.20	0.50	0.12
Manganese	15.25	15.25	15.25
Siliceous earth	2.25	0.60	0.175
Iron	80.30	83.65	84.45

In manganese determinations Bergman evidently took care to avoid the discrepancies which are said to characterize the work of modern chemists, for we find that his manganese percentage is in each case 15.25 per cent. This celebrated chemist confirmed, to his own satisfaction, the conclusions of Réaumur (1683 to 1757), who considered steel an intermediate grade of metal between crude and malleable iron. His experiments showed that malleable iron left the smallest quantity of insoluble residue, steel a greater quantity, and cast iron the greatest of all, and from this he drew his conclusions with respect to the difference between iron, steel and cast iron. "Nothing more was necessary," says Thomson, "than to apply the anti-phlogistic theory to these experiments, as was done some time after by the French chemists, in order to draw important conclusions respecting the nature of these bodies. Iron is a simple body, steel is a compound of iron and carbon, and cast iron of iron and a still greater proportion of carbon. The defective part of the experiments of Bergman, as recorded in this important paper, is his method of determining the manganese in iron. In some specimens he makes manganese to amount to considerably more than one-third part of the whole. Now, we know," continues Thomson, "that a mixture of two parts of iron and one of manganese is brittle and useless. We are therefore sure that no malleable iron whatever can contain any such proportion of manganese. The fact is, that Bergman's method of separating iron ores was defective. What he considered manganese was chiefly, and might be in many cases altogether, oxide of iron. Many years elapsed before a good process for separating iron from manganese was discovered." To this I may add that many more years elapsed before steel containing 30 per cent. of manganese, of which some description will be given in one of the papers to be read at this meeting, became a commercial product.

Among other investigations by Bergman were a series of experiments made by him with a view to ascertaining the cause of brittleness in cold-short iron. He extracted from such iron a white powder, by dissolving it in sulphuric acid. This white powder he succeeded in reducing to a white and brittle metal, by fusing it with a flux and charcoal. Klaproth (1743 to 1817), soon after described this metal as a phosphuret of iron, and Scheele, with his usual sagacity, hit on a method of analyzing it, and thus demonstrating its nature. Meyer seems to have conducted a line of experiments in the same direction about the time of Bergman's work, and he made his conclusions known to chemists in time to dispute with Bergman a claim to priority of discovery. As may be supposed, Bergman's processes were rude and very imperfect. It was Klaproth who first systematized chemical analysis, and brought the art to such a state that the processes could be imitated by others with nearly the same results in each case. Klaproth analyzed about 200 specimens of minerals and metals, and most of his conclusions were so

nearly correct that his successors have, in most cases, confirmed the results he obtained. When he began his researches, chemists were not acquainted with the true composition of a single mineral substance. The service which Klaproth performed for mineralogy in Germany was performed equally well in France by Vauquelin (1763 to 1829). To this chemist we are indebted for a description of the element chromium. All of the early analyses of ores, iron and steel are credited to one or the other of these two chemists. Vauquelin announced that in steel the carbon percentage averaged $\frac{1}{140}$th part. By inclosing diamonds in cavities of soft iron and igniting them, they disappeared, and the inner surface of the cavity was found to be converted into steel. I am not aware that this process is employed at the present time, but judging from the disproportion frequently noted in experimental steel manufacture, between the cost and value of the product, one might suppose it is still in use. Berzelius, in the first quarter of the present century, and Ebelmen, about ten years later, made important contributions to the knowledge of reagents and methods. Berzelius was the successor of Bergman and Scheele. All previous analyses were revised by him, and modern chemistry begins with his era. One of his iron analyses shows iron 90.80, silicium 0.50, magnesium 0.20, manganese, 4.57, carbon, 3.90. The pupils of Berzilius were, to a great extent, instructors of the chemists of to-day.

Karsten, in 1820, recognized the influence of carbon on iron, and stated his belief that iron and steel constitute a continuous series, there being no distinct lines of separation between them. In his judgment it was simply a question of carbon percentage where, in the series, a piece of iron or steel belonged. In his "Metallurgy," published in 1830, he notes the fact that pig iron contains carbon, silicon, sulphur, phosphorus, manganese, calcium, magnesium and chromium. It is probable, however, that all these elements had been previously recognized and described. As early as 1815 there was more or less speculation whether hardness and softness in steel were due to physical or chemical causes. Faraday is credited by Percy with having been, in 1822, the first to point out that a piece of hardened steel dissolved completely in hydrochloric acid, while soft steel always yielded a certain amount of carbonaceous residue when subjected to the action of that solvent. David Mushet, in *Iron and Steel* (1840), gives a very good idea of what was known of metallurgical chemistry at that time. He mentions certain ores which contain "phosphat" of iron, which was generally believed to account for the fact that the iron made from them was cold-short. Mushet, however, was by no means certain of the cause of cold-shortness. Phosphorus, he tells us, had long been regarded as the prime cause of this quality in iron, but by the practical observer this theory could not be considered tenable, for it had always been noticed that the most perfect qualities of iron, notably some of the Swedish makes, gave out in working "a very strong phosphoric smell." Regarding the condition in which carbon exists in iron, Mushet says, "In the works of those who have treated on iron, I have never yet seen carbon which exists in crude iron, distinguished from that absorbed by malleable iron in the process of converting it into steel. I could," he continues, "adduce many facts which to me appear conclusive, to prove that carbon exists in crude iron in a concrete state separable by mechanical division, and that it is united to steel in a gaseous state by the combustion of its base, inseparable in any form by the most minute mechanical reduction." It is surprising to note the earnestness and gravity with which in 1840, these statements were made. It shows the newness of the knowledge which now-a-days serves as the starting-point for discussion on such topics.

Mushet treats very fully of the effect of different substances on the quality of iron. He made a number of experiments by fusing iron with different fluxes in crucibles, and noting the quality of metal produced. One section of his book is devoted to the different proportions of carbon which constitute iron and steel; and he gives the results of fourteen experiments. His method was to fuse a certain number of grains of wrought iron with charcoal in varying proportions and note the increase of weight as showing the amount of carbon taken up. Karsten, however, promptly challenged the accuracy of his methods, and proceeded to

show that Mushet's tables giving the carbon percentages in iron and steel were entirely wrong—much as chemists of the present day are prone to do upon occasion.

It is unnecessary to follow from this point the progress of metallurgical chemistry towards a scientific basis. Its general employment as a means of assisting makers to control the character of their product concerns us more; and this is almost within the memory of even the youngest of our membership. Most of us can recollect when the dependence of the iron-master and the engineer who cared to know the chemical composition of a piece of iron or steel, was upon the general analytical chemist. When the influence of our technical schools began to be felt, and young men well equipped for the work began to displace, in the management of furnaces and mills, those who had gained their knowledge in the school of experience, where the instruction is not always thorough in proportion to the cost of tuition, the laboratory began to be recognized as an essential part of an iron or steel-making plant, and in nearly every establishment with any pretensions to completeness, the chemist has become an important member of the staff. But it is not more than fourteen years ago that this was the exception rather than the rule. Among my letters I have one bearing date of 1872, written by the general manager of an important iron-works. He says: "The president of our company thinks we ought to follow the fashion and have a chemist. To my mind it is a waste of money. When I want an analysis I can have it made—and that is very seldom; for the furnace-manager who needs a chemist to tell him the quality of ore or limestone, or whether his pig iron is soft or hard, had better resign and go to farming. However, if the president says chemist, chemist it is. My object in writing is to know if you can recommend a young man competent to fit up a laboratory and take charge of it. We have very little society here, and it is desirable that he should be a gentleman. My wife plays the piano, and I do a little on the flute, and if we can get a chemist who plays the violin, we could have some music evenings. If you can suggest a man who combines these qualifications I could employ him. I do not know what a chemist would expect, but I should not care to pay more than $10 a week."

When the demand for analytical work in connection with the iron and steel industry began to be felt, it brought into the service of the iron-master a great many clever and ingenious chemists at home and abroad, and a varied and valuable literature of metallurgical chemistry was soon created. The need of accurate analyses was so evident that their importance was, perhaps, somewhat exaggerated and for a time it seemed as if we might safely look to the chemist to answer every question which could be raised by the iron-master or the engineer. Our confidence in tabulated percentages of the component parts of a piece of iron or steel resulted largely from the fact that we knew so little what knowledge was needed for a clear and satisfactory explanation of observed phenomena. From this over-confidence in the power of the chemist to explain everything, there has been a natural, and doubtless wholesome, reaction. Experience has shown that, great as the value of a knowledge of the chemical composition of a piece of metal may be, it is, after all, only a part of the knowledge we need before we can determine with what we are dealing.

To some extent, coincident with this rapid progress of chemical investigation, and within even a shorter period, we have seen the development of the physical test, with the aid of appliances which have attained marvelous perfection in surprisingly few years.

Thomas Tredgold, in his "Strength of Cast Iron," published in 1823, says: "Lord Bacon's idea of a mechanical history, which Diderot attempted to realize, is not so well calculated to fulfill his own views (concerning the advancement of the arts) as a well-directed course of experiments on the nature, forms and properties of materials. . . . . In chemistry much has been done, but an experimental school of mechanical science remains to be formed." Referring to the necessity for more knowledge of physical properties than was at that time possessed, Tredgold says: "The manner in which the resistance of materials has been treated by most of our common mechanical writers has also, in some sense, misled the practical men who are

desirous of proceeding upon sure ground, and has given occasion for the sarcastic remark that the stability of a building is inversely proportional to the science of the builder."

Coulomb, in 1784, made some important experiments on torsion, and was probably among the first to study the effect of continued stress upon the elastic limit of iron and steel. In 1818 Wilson estimated the power required to crush cast iron at 2,240,000 pounds to the cubic inch. Reynolds, quoted by Wilson, recorded an experiment in which a cube of cast iron one-fourth of an inch square, required 448,000 pounds to crush it. Tredgold considered it necessary to correct these erroneous estimates, and made numerous experiments with cast iron, testing specimens by static loads and under a drop. The results are given in the work before mentioned. He also made some experiments upon wrought iron, correcting or verifying the results reached with crude methods by various European experimenters between 1758 and 1820. The modulus of elasticity of steel was probably first calculated by Dr. Thomas Young, about 1820, from the vibrations of a tuning fork. The height of a modulus found by this method was 8,830,000 feet, and the weight per square inch was 29,000,000 pounds. That is, a bar of steel 8,830,000 feet in length, and 1 inch square in cross section, would stretch from its own weight to double its original length; and its weight, 29,000,000 pounds, is the modulus of elasticity as ordinarily expressed.

It is within a century that the work of Navier, Perronet, Poleni, Telford, Brunel, and others furnished the basis for a more or less exact knowledge of some of the more easily recognized and described physical properties of iron and steel. Naturally the results reached by these experimenters were as incomplete, and in many instances as mistaken, as their methods and appliances were rude and unsatisfactory. Drop-hammers, single lever testing machines and hydraulic presses were the only power appliances employed in testing during the first half of the present century. Experiments were mostly directed to ascertaining the tensile strength of materials, chiefly iron, steel and wood, under shocks or stresses which, at a single application, would produce rupture. The breaking point thus ascertained was termed the ultimate strength of the material; and, until very recently the data thus gathered were the only bases for calculating the dimensions of members which were expected to resist tension. Resistance to compression was similarly determined by the application of crushing loads to cubes of unit dimensions; and this was deemed satisfactory until the experiments of Hodgkinson demonstrated the previously unrecognized influence upon resistance to compression of the ratio of diameter to length in test specimens.

Among the earlier of the experimenters in this field, Navier is entitled to special prominence. He probably did more than any one else to bring science and practice together, and to make one help the other. Navier's theory of rupture under transverse strain, though since found to be correct only within certain limits, is still quite generally accepted as a basis for calculations dealing with such strains. To Woehler, in 1858, we are indebted for a knowledge of the influence of the repetition of quiescent stresses. This led to the formulation of Woehler's law, that rupture may be caused by the frequent application of stresses, in no instance approximating the original ultimate strength of the metal. The recognition of this law established the significance of the elastic limit in the calculation of dimensions, and marks what is probably the most important epoch in modern methods of dimensioning.

In 1862, Kirkaldy published his "Results of an Experimental Inquiry, etc.," which effected a considerable modification of the views previously held by engineers as to the physical characteristics of materials, especially of steel. These investigations tended in a material degree to popularize experiments with construction materials in the testing machine, and created a demand for such machines, and for accessory apparatus for measuring elongation, etc. The Messrs. Fairbanks were, I believe, the first to produce, in 1863, a testing machine on the multiple-lever principle, and though of limited capacity, this was an important improvement upon previous constructions. They were quickly followed by Riehlé Bros., whose testing ma-

chines still hold a high place in the estimation of experimenters. The next great step forward was marked by the production of Thurston's automatic recording torsional testing machine. The progress continued until it culminated in the Emery testing machine, probably the most remarkable instrument of precision ever built, and the most improved type of which dates no further back than 1880. In the line of automatic recording apparatus the latest form, devised by Abbott, illustrates the high development attained in the construction of testing machine accessories.

The period from 1850 to 1875 was, without doubt, the most fruitful in additions to our knowledge of the physical properties of iron and steel as revealed by the testing machine. It comprised the investigations of Navier, Fairbairn, Woehler, Spangenberg, Kirkaldy and Thurston. The work of these and other investigators, brought the physical laboratory fully abreast with the chemical laboratory, and each has given to work done in the other a value it would not othewise have possessed. But he who should undertake the study of iron and steel with no other light than that which analysis and test can give him, though he would learn much of value, would find himself baffled at every turn by mysteries which these methods of investigation cannot solve. This is especially true of steel. In my experience, very few of those who make or use steel are prepared to accept the statement that chemical analysis alone can be relied upon to determine its quality. It may be broadly stated that certain compositions never make good steel; but the reverse cannot be asserted with equal confidence. With a given composition the result depends primarily upon the perfect admixture of the ingredients. Imperfect melting will give an unsatisfactory product, no matter what the stock used or the composition shown by analysis of the ingot. It will also be questioned by many whether a method of accurately determining the oxide of iron in steel would materially increase the confidence we should feel in a judgment of quality from analysis. It is true that chemical methods are becoming more rapid and accurate every day; but with as complete a knowledge of the stock from which steel is made as chemical analysis can give us, there still remain a great many uncertain factors in the equation of quality. In fact, it seems that the value of ingot analysis may easily be very much exaggerated, and that within certain limits the physical structure of a piece of metal is quite as important to be known as its chemical composition. There are many gentlemen in this audience who could substantiate, by the results of long experience, the broad statement, that without good melting and proper subsequent treatment of the ingot, good steel is impossible with any admixture of ingredients which the chemist may prescribe. Chemistry has its limitations—not quite sharply defined, perhaps, but still evident. If the chemist should ever succeed in giving us a report showing the exact proportion of each constituent of a piece of metal, beyond question or doubt, we might still be in the work before us where the builder is when he stands among his bricks and lumber and the sundry materials he makes use of in construction. If he knew nothing more than the count and tally of his materials he could build nothing.

Nor is the testing machine infallible. What it shows is, to a greater or less extent, dependent upon what the operator seeks to have it show. We all know how, on the one hand, by sudden shock applied to a specimen under stress, it can be made to give results far below any recognized standard; and how, on the other hand, by gentle increments of stress through lengthening intervals of time a piece of metal may be coaxed to show test results far above its real value, and apparently inconsistent with it chemical composition. But even when it is possible to have such confidence in a physical test as can only result from a knowledge that everything connected with it has been honest and fair, and surrounded by safeguards against every known source of error, we must look elsewhere than to the chemist for an explanation of many of the phenomena which the testing machine reveals. If we seek to compare the results of physical and chemical test we shall become hopelessly confused. Generalizations warranted by one relation of composition to qualify will often be contradicted by a different relation, and we should reach the almost despairing conclusion that one or the

other of these methods must be accepted as the sole standard by which to judge quality. Which we should choose would depend upon whether we had formed our opinions in the laboratory or in the mill.

To harmonize what seem to be the often-conflicting results of chemical analysis and physical test, we must seek for a knowledge of causes affecting quality in yet other directions. In this we have already had assistance of the greatest value. The anomalies developed by the steels made by different formulæ, and of samples of a given composition taken at different stages in the process of manufacture, has called attention to the fact that the quality of steel does not alone depend upon what it is made of.

In 1873 the building of three large iron-clads was begun at Brest and L'Orient, and steel was largely used in their construction. Lieut. J. Barba, Chief Naval Constructor at L'Orient, investigated some of these anomalies, and to him we are indebted for the first exact observation of the effect of manipulations upon steel. Barba's work was ably supplemented by that of Joessell, and continued by Pourcel, Holly, Metcalf, Hill, and others, with the result of showing that the influence of manipulation in processes of manufacture is of prime importance in its relation to the quality of the finished product. It is important to know the chemical composition of muck-bar and ingot, but experiment and experience have shown that the beam, the rail, the ship-plate and the bridge-member on which the safety of the whole structure may depend, may be so far below the standard of quality which analysis would lead us to expect, or physical tests of specimens taken at intermediate stages in the process of manufacture warrant us in assuming, that we must seek still further light on the subject in the revelations of microscopic analysis.

In the microscope we have an instrument which promises to supplement the laboratory and the testing machine, to harmonize their seemingly conflicting records, and to detect the influence of shop treatment at every step in the process of manufacture. The work of the microscopists who have thus far turned their attention to iron and steel, has not been complete enough as yet to give us more than a few standards by which to compare our observations, but I do not doubt that within a very few years the microscope will give the laboratory and the testing machine a value for the iron-master, the steelmaker and the engineer incomparably greater than that they now possess. This branch of special investigation is one which offers many attractions for the conscientious student who will approach it in the earnest spirit of scientific inquiry. Within the little circle of the field of a microscope, there is more to be learned of value to science and the arts than the chemist can predict or the physicist explain. It will bring us to the point beyond which no investigation can proceed. Then, as now, we shall realize that "the utmost still is hid;" but when we shall have learned all it is possible to learn from the revelations of the microscope, we shall have followed truth to the limit of human intelligence, and seen it fade into infinite mystery.

Meanwhile, let us remember how new is our knowledge of iron and steel, how incomplete, and how dependent is the student by one method upon the knowledge gained by other methods, some as yet almost untried. It is too soon for broad generalizations. The key to the mystery seems to lie in the structure of the metal, and until we know more of this, and can reason from effect to cause through the known phenomena of analysis and test, we may safely distrust that assurance of conviction and positiveness of utterance which Tyndall tells us are ever characteristic of the "confidence of half knowledge."

THE Japanese are seriously considering the utilization of the hot springs near Tokio, as a means of producing heat and power. The subject has been discussed in the Japanese Scismologic Society. In a country where the presence of hot springs and the frequency of earthquakes indicate a rapid increase of subterranean temperature, the thing may be quite practicable. In the "Proceedings" of the Paris Society of Civil Engineers it has even been questioned whether the extraction of the heat would not diminish in some degree the frequency and intensity of earthquakes; but if any considerable heat extraction could be effected, exactly the contrary would be the result.

ON THE REDUCTION OF THE GRADIENT ON CURVES.

By H. P. VINCENT.

Contributed to Van Nostrand's Engineering Magazine.

In the following paper are contained the results of an investigation into the nature of the resistance to the movement of trains, arising from curves. The older data, on which it is based, are indeed but "rudely observed facts," as so forcibly pointed out by Mr. Wellington in his celebrated work. There is, however, also a small body of later observations, both published and unpublished, besides my own, of which I have availed myself. Altogether the material is scanty, considering the importance of the subject; and it would therefore be too much to expect a fine degree of accuracy in the resulting formula.

Let Q = number of horse-powers developed by the locomotive in taking a train at uniform speed up any grade—provided that grade be perfectly straight.

P = number of horse-powers developed by the locomotive in taking the same train, at the same uniform speed, up that same grade—provided that grade be all on a curve of the degree d.

d = degree of curve.

n = total number of cars (locomotive and tender included) which constitute the train under consideration.

L = total length of that train.

k = coefficient of curve-resistance; not to be confounded with the coefficient of friction. The coefficient of friction for the different materials have been ascertained under the condition, that the pressure should never become large enough to cause noticeable abrasion. But it is the very nature of curve-resistance to cause abrasion.

Then there is approximately,

$$P = \frac{(1+t)^{n+1} - (1+t)}{nt} Q \quad \text{where}$$

$$t = \frac{d\,L}{5730\ n} k$$

This formula is based on the car as a unit. Better results, at least theoretically, may be expected from introducing, as a unit, the axle, or even the rigid wheel-base. Whether the practical benefit will compensate for the increased difficulty of the formula I have not, at present, the means of deciding, but intend to consider at some future day.

To utilize this formula for freight service—the only one I shall here consider—for the purpose of finding the grade g (in per cent.) which, combined with the curve of degree d, shall consume the same power as the tangent grade G (in per cent.), I assume the total resistance of the train on straight and level track, for the usual speed of trains (10 miles per hour) as 8 pounds per ton (@ 2,000 lbs.) of the weight of train. This resistance is greater than the so-called rolling friction of the train. The former is the measure of the work performed by the engine; the latter alone taxes the adhesion. The object here to be obtained is to equalize, not merely the external resistances, but the total work of the engine. This value is but an approximation by Clark's data. There are other formulæ, more complicated, but scarcely more trustworthy. Nothing can give accurate values but an extensive study of indicator diagrams taken under the carefully ascertained conditions of actual traffic.

With this assumption there is

$$g = (G + 0.4) \frac{nt}{(1+t)^{n+1} - (1+t)} - 0.4 \quad \text{where}$$

$$t=\frac{d\mathrm{L}}{5730\ n}(0.3+0.06d)$$

The value of the coefficient k, as here given, is the weak spot of this formula, for the reasons pointed out in the introduction. Such as it is, it gives fair working results up to 20° or 21° curves. The additional resistance which may be caused by the centrifugal force of the train, is not included. For roads with trains of low average speeds I simply neglect it; and for roads with high average speeds I assume it counterbalanced by the super-elevation. In the latter case the use of some kind of transition curve is essential.

L and n depend principally upon the locomotive used; upon the weight of the average car, and upon the weight of its average load. In general, n should be so taken, that the locomotive could just move the train at normal speed from water-station to water-station up the grade under consideration, if it were perfectly straight.

This formula is intended only for the reduction of the grade on curves, which are longer than the train, and for standard gauge roads. For shorter curves a formula might also be given, but it is simpler in practice to use the reduced grade on all curves, long and short alike; but on curves shorter than the length L of train, instead of beginning and ending the reduced grade at the nearest station, resp. half-station to the B. C. and E. C., begin and end it at another station, resp. half-station, further on the curve, in inverse proportion to its length.

As an example, I will take the Mogul locomotives of the Southern Pacific Railroad, of 64,000 lbs., weight on the drivers, total weight with tender, about 120,000 lbs, boiler pressure, 120 lbs. per square inch. It can, with certainty, take 20 average loaded cars up a 1 per cent. straight grade at a speed of twelve miles per hour, at a continuous effect of 412 indicated horse-powers.

The following table shows the grades, which, combined with the curves above them, will require the same continuous effect from the engine, provided that all curves are at least as long as the train. For shorter curves an approximate method has been indicated above; and instead of returning to the full tangent grade for shorter distances than 200 feet, the grade on one curve should gradually pass into that on the other. As a matter of course, abrupt changes of grades are to be eased off by the usual vertical curves.

Degree of curve.....	0	1	2	3	4	5	6	7	8
Grade in per cent.....	1.000	0.973	0.935	0.888	0.833	0.771	0.703	0.630	0.553
Difference.....		27	38	47	55	62	68	73	77

Degree of curve.....	9	10	11	12	13	14	15	16
Grade in per cent.....	0.473	0.393	0.313	0.235	0.160	0.089	0.023	0.037 descend's
Difference.....	80	80	80	78	75	71	66	60

The third line contains the differences between the grades in thousandths of one per cent. It will be noticed, that these differences are increasing at first, are then stationary and finally are decreasing. This is an expression of the fact, that the value of the coefficient k is but an approximation. It is easy to see, even without graphical representation, that about a $14\frac{1}{2}$° curve on a level would be equivalent to a straight 1 per cent. grade for the locomotive here considered, with such weight of average car and average load, as form the basis of this computation.

Any one objecting, that this table shows too much of a reduction of the gradient, could readily use smaller proportionate amounts, if so inclined. And

even to those, who—rejecting formula altogether—use empirical scales, this and corresponding tables might serve as a kind of preliminary guide. For empirical scales can by much hard work be made, which will give excellent results, as experiments carried out on the more recently built portions of the Southern Pacific Railroad amply prove. The grades on curves on this road are somewhat less reduced than the above table shows, but the whole system was undoubtedly designed originally for a lighter locomotive—probably of the Standard American pattern—than the one there largely used now for heavy freight trains. The methods above advocated, however, are not those of the Southern Pacific. The latter are arithmetically more accurate, but also more laborious.

It has been objected that this investigation rests upon no basis of experimental fact; that the existing state of knowledge does not warrant the use of so complicated a formula; and that it probably contains a theoretical error.

These objections seem to involve an entire misconception of the meaning of a formula, such as the above. This is, like every other physico-mathematical theory, an attempt to combine and harmonize such observations as are available. It is indeed very much to be regretted that the same interest is not manifested in the locomotive that has been lately exhibited in the mechanical movements of the horse, for which the wonderful series of experiments in California form the basis of all accurate knowledge. For then we would now have, or might soon expect to obtain, a large body of scientific experiments on the performance of the locomotive. But as there is no immediate hope of this, the best any one can do is to combine and utilize such observations as there are. An apprehension, that this formula contains a theoretical error, is saying but little against it. I think it very likely that it *does* contain a theoretical error, and would consider it little short of a miracle if it did not. The apparent complication of the formula is nothing against its value, but merely against its convenience. It is in the nature of things that a *simple* formula *cannot* be correct. Besides, the former assumption of a reduction of the gradient in direct proportion to the sharpness of the curve, on all gradients alike does not lead to a *simple* formula for the work performed by the engine, which is after all the fundamental consideration. And less so does the progressive scale given in Mr. Wellington's work. That these scales were never before put into the shape of formulæ does not alter the matter: the mathematical law remains the same, however expressed.

To compare this subject to another of vastly greater importance—the flow of water in channels. For nearly a century a great number of mathematical expressions for the velocity of flow have been given by different investigators. Which of them is the correct one? There is evidently *no* correct one; for every one of them, under certain conditions, gives notoriously erroneous results. And when Messrs. Ganguillet and Kutter published their investigation, was the state of knowledge at the time such as to warrant the use of *that* complicated formula? Nevertheless, very many hydraulicians seem to think that it has helped to advance our knowledge and that it is, in fact, the best in existence.

In view of this and other similar cases, I have no fear of corrupting the scientific conscience of my fellow "railroad-stakers-out" and venture to submit to them my formula with all its imperfections on its head.

A NEW process for working lead fume into litharge and red lead has been described in the "Journal" of the Society of Chemical Industry. The fumes evolved from the working of galena contain lead sulphate, sulphite and oxide, arsenic and antimony, also lead sulphide, and when zinc ores are present, zinc oxide. The lead fume is mixed with sodium carbonate or hydroxide, and roasted. The roasted product is then washed, whereby sodium sulphate and sulphite and sodium compounds, containing arsenic and antimony, are separated. The lead compounds are converted into lead oxide by this treatment. The lead fume may be boiled also with a solution of sodium carbonate or hydroxide, lead carbonate and hydroxide being formed, whilst arsenic and antimony are dissolved. The washed precipitate is then roasted. In the presence of zinc compounds they are removed by boiling with sulphuric acid. If lead sulphide be present, it is necessary to boil first with a solution of calcium hypochlorite. Sodium sulphate is recovered from the liquors after separating arsenic and antimony.

PRIMARY BATTERIES FOR ELECTRIC LIGHTING.

By ISAAC PROBERT.

From the "Journal of the Society of Arts."

It has been said that "history repeats itself," and this quotation was never more appropriate than to the subject that will engage our attention this evening.

All of us probably know that it was by a primary battery that the electric light was first introduced. So far back as 1802, Sir Humphry Davy, using a battery consisting of plates of copper and zinc dipped into dilute acid, obtained the electric arc between poles of carbon. In the *Journal of the Royal Institution* for that year, Davy, in describing some experiments on the spark yielded by the newly-invented galvanic battery, uses these words:—"When, instead of the metals, pieces of well-burned charcoal were employed, the spark was still larger, and of a vivid whiteness." He also pictures an apparatus for, as he says, "taking the galvanic electrical spark in fluids and aeriform substances." It consisted of a glass tube, open at the top, and having a tubular outlet at the side through which a wire tipped with charcoal was introduced, another wire, also tipped with charcoal, being cemented in a vertical position through the bottom.

In the same year, the electric light, sustained by voltaic action, was publicly shown in Paris by the citizen Robertson. Professor Sylvanus P. Thompson, in an interesting communication to *Nature*, has quoted a passage in the *Paris Journal* of March, 1802, from which it appears that the experimenter used a voltaic pile of 120 elements of zinc and silver, to each pole of which he attached a carbon. On bringing the carbons into contact, a brilliant spark was obtained of extreme whiteness.

A few years later, Davy, with the large battery of 2,000 cells, which the munificence of some members of the Royal Institution placed at his disposal, obtained an electric light of a power that has rarely been surpassed, even in our own day.

Thanks to the kind efforts of our Chairman, one of Davy's original battery cells is on the table to-night. The complete battery, to use Davy's own words, consisted "of 200 instruments connected together in regular order, each composed of 10 double plates arranged in cells of porcelain, and containing in each plate 32 square inches. The battery, when the cells were filled with 60 parts of water, mixed with one part of nitric acid, and one part of sulphuric acid, afforded a series of brilliant and impressive effects. When pieces of charcoal about an inch long and one-sixth of an inch in diameter, were brought near each other (within the thirtieth or fortieth part of an inch), a bright spark was produced, and more than half the volume of the charcoal became ignited to whiteness, and by withdrawing the points from each other, a constant discharge took place through the heated air, in a space equal to at least four inches, producing a most brilliant ascending arch of light, broad and conical in form in the middle."

The electrician, Children, constructed a battery of twenty cells having huge double plates four feet by two, of which the whole surfaces were exposed, in a wooden trough, in cells covered with cement, to the action of diluted acids. It was the grandest combination ever then constructed for exhibiting the effects of extensive surface. To quote Davy's words:—"Points of charcoal ignited by it produced a light so vivid, that even the sunshine, compared with it, appeared feeble."

The light obtained by these experimenters was beautiful in the extreme, and naturally excited the admiration and the hopes of the public, or at least of that educated section of the public to whom the advances of science are not unknown; but it was in no way a practical light. The copper-zinc battery used in the experiments was crude, and quite incapable of sustaining a constant current, the carbon points employed for the arc to play between were mere sticks of wood charcoal, which gave an unsteady light and rapidly consumed, and the beautiful regulators with which we are familiar, for

keeping the length of the arc constant, were then quite unknown.

It has just been said that the current obtained by the battery employed in those early experiments was not constant. This want of constancy is due to what is called the polarization of the battery—that is, the deposition of hydrogen gas upon the copper plate. In the working of the battery, the zinc plate is oxidized and transformed into sulphate of zinc by the sulphuric acid in which the plates are immersed. At the same time, hydrogen gas is liberated on the surface of the copper-plate, where it collects and forms a layer of appreciable thickness, which offers considerable resistance to the transmission of the electric current. It also gives rise to an electro-motive force opposed in direction to that of the copper zinc couple. The effect of the polarization of the battery is then to considerably reduce the current flowing, and worse than this, the amount of deposited gas varies with time and other circumstances, and the current likewise varies Such a battery, when used to produce the electric light, furnishes a light of constantly varying intensity which is extremely painful to use, and possesses other drawbacks also.

It is evident then that a battery, to be of any use for practical electric lighting, must be capable of generating a constant current. It should also possess these other qualifications, viz., a high and constant electromotive force and a small and constant internal resistance. It should consume inexpensive materials, and should consume nothing when it is producing no current. It should be capable of being easily cleaned, and of being supplied with fresh materials. It should require no skilled attendance. Unfortunately, no battery hitherto invented possesses all the required qualifications.

So long as constant batteries were unknown, the electric light remained a scientific curiosity, but the invention, by Grove, in 1836, of his nitric acid battery, gave a fresh impetus to the subject. Of all the points enumerated, the power to furnish a constant current is the most important. As the want of constancy of a battery is due very largely, although not exclusively, to the deposition of hydrogen gas upon the copper plate, inventors naturally turn their attention to the devising of methods of ridding the battery of this objectionable feature. Mr. Alfred Smee was very early in the field; he found that by roughening the copper plate, the disengagement of the bubbles of hydrogen from its surface was greatly facilitated. The gas, instead of forming a smooth layer upon the copper, collected upon the rough portions, and was discharged upwards through the liquid in streams.

On further investigating the subject, Smee found that it was not necessary that these roughenings of the surface should have appreciable size. In fact, better results were obtained with a multitude of small points, from which the gas could be disengaged as quickly as it was liberated by the electrolytic action of the battery, than with a few points of large size, which, although capable of releasing big bubbles, yet permitted the gas to remain on the plate until the big bubbles had been formed by the aggregation of the tiny particles of gas which first appear. He therefore abandoned the copper plate, roughened mechanically, with which his earlier experiments were performed, in favor of a platinum plate, coated with a deposit of very finely divided platinum, obtained by exposing the plate to the action of solution of chloride of the metal, under the influence of the galvanic current. In our own day, platinized silver is often, through a false idea of economy, substituted for the platinized platinum.

This was a very great advance. The Smee battery was justly esteemed at the time for its great constancy as compared with the batteries previously in use. But its constancy, although great in comparison with that of the other batteries then known, was still very far from perfect.

Smee's method of eliminating the ill-effects of the layer of hydrogen was but a method of removal, and the question naturally presented itself to the minds of inventors whether a method of prevention could not be devised. An answer in the affirmative was given by Professor J. F. Daniell, of King's College, who invented a battery which remains to this day unequaled in constancy. You may be interested to hear the description of this first constant battery worthy of the name, in its first form, in the inventor's own words. They are these:

"A cell of this battery consists of a cylinder of copper three and a-half inches in diameter, which experience has proved to afford the most advantageous distance between the generating (that is the zinc) and conducting (that is the copper) surfaces, but which may vary in height according to the power which it is wished to obtain. A membranous tube, formed of the gullet of an ox, is hung in the center by a collar and circular copper plate, resting upon a rim placed near the top of the cylinder, and in this is suspended, by a wooden cross bar, a cylindrical rod of amalgamated zinc, half an inch in diameter. The cell is charged with a mixture of eight parts of water and one of oil of vitriol, which has been saturated with sulphate of copper; and portions of the solid salt are placed upon the upper copper plate, which is perforated like a colander, for the purpose of keeping the solution always in a state of saturation. The internal tube is filled with the same acid mixture without the copper. A tube of porous earthenware may be substituted for the membrane with some little loss of power. A number of such cells admit of being connected together very readily into a compound circuit, and will sustain a perfectly equal and steady current for many hours together, with a power far beyond that which can be produced by any other arrangement of a similar quantity of the same metals.

"The surface of the conducting metal is thus perpetually renewed by the deposition of pure copper, and the counteraction of zinc or any other precipitated metal effectually prevented. The minor affinity of the copper for the acid, however, still remains, and such an opposition could only be effectually avoided by the employment of platinum plates, perpetually renewed by the decomposition in the circuit of chloride of platinum; such an arrangement would be perfect, but too costly for ordinary applications."

This battery, the prototype of all chemical depolarizing batteries, is worthy of attentive study. In it the hydrogen, instead of being deposited upon the copper plate, is oxidized to water by the solution of copper sulphate, and metallic copper in place of hydrogen is thrown down upon the copper plate. Polarization is thus entirely prevented.

If constancy of current were the sole requisite of a battery for electric lighting, the Daniell battery would be admirably suited to the purpose, but it will be remembered that a high electromotive force and a low internal resistance are also essential, and here the Daniell battery fails. The electromotive force of any copper zinc couple is low, and the internal resistance of the Daniell cell is high. The electromotive force might be raised by the device suggested by Daniell of employing platinum in place of copper, and solution of chloride of platinum in place of solution of sulphate of copper, but the expense would militate against the practical success of such an arrangement.

It was reserved for Grove—now the Hon. Sir W. R. Grove, one of her Majesty's Judges, but then Professor of Chemistry at the London Institution—to overcome this difficulty, and to produce a chemical depolarizing battery of high electromotive force, low internal resistance, and employing a comparative cheap depolarizing liquid. Daniell, as we have seen, had appreciated very clearly the advantage of a platinum plate, but his mind seemed so engrossed with his original idea of a metallic solution, as depolarizer, which in decomposition deposit upon the plate dipping into it a layer of the same metal, that the possibility of finding a non-metallic depolarizer does not appear to have suggested itself to him. Grove, however, saw this possibility, and by the substitution of strong nitric acid for the metallic solution, achieved the success which Daniell had vainly sought for. For the first time, the production of the electric light by the acid of a primary battery became possible on a practical scale; and the very year of Grove's invention, the electric light was used for theatrical purposes, at the Opera House, in Paris.

Suggestions by Cooper, Walker, Bunsen, and Archereau, quickly followed, and in the course of a few years the nitric acid battery had assumed its present convenient form.

It would be tedious to tell you of the many attempts to practically introduce the electric light which now rapidly succeeded one another. One of the most noteworthy instances of the employment of this battery is the electric lighting of the Cherbourg Docks, during their construction in 1858, whereby 1,800 men

were enabled to work during the night, when work must otherwise have been suspended.

In 1831, Faraday made the important discovery that when a closed circuit of conducting material—a ring of copper, for example—is moved in the neighborhood of a magnet, so that the conductor passes from a position of one magnetic intensity to a position of a different magnetic intensity, a current of electricity is generated in the conductor, and that when the conductor is moved, so that a current is generated in it, more mechanical work is done in producing the motion than when no current is generated. In effect, this great discovery was that, by the intervention of a magnet and a coil of wire, mechanical energy—the energy of the arm or of the steam-engine—can be transferred into electric energy. Out of this discovery have grown the huge dynamo-electric machines of to-day, which, as generators of electricity for lighting purposes, have almost driven voltaic batteries from the field of competition.

Why is this? Why is it that a dynamo-electric machine should be almost universally preferred to a voltaic battery for the production of the electric light? It is a question of cost—of the relative cost of the two methods of producing an electric current. Let us examine this point a little in detail.

Both in a voltaic battery and in a dynamo-electric machine, the electric energy is ultimately referable to combustion, or, more strictly, to chemical combustion. In each there is a combustible substance, oxygen or an equivalent substance, to sustain the combustion, and the means wherewith to collect and utilize the energy liberated. In a voltaic battery, the combustible substance is usually zinc, and the oxygenous substance sulphuric acid. In a dynamo-electric machine—or rather in the furnace of the engine that drives it—the combustible substance is coal, and the oxygenous substance air. The question, therefore, as ordinarily presented, resolves itself into this: Required a certain quantity of electric energy; can it be more economically produced by burning zinc in sulphuric acid in a voltaic battery, or by burning coal in air in the furnace of the engine of a dynamo-electric machine? In answering this question, we must consider the relative cost of zinc and coal, their thermal equivalents—that is, the amounts of heat obtainable by the combustion of equal weights of the two materials; the efficiency of the battery and the engine and machine—that is, the proportion of useful work obtained to the total energy generated; the cost of the oxidant of the zinc-air—the oxidant of the coal costing nothing; the relative cost, both original and for maintenance of the battery and the dynamo and engine; and the relative cost of attendance. In the case of the battery, there is also the possibility of the utilization of the bye-products to be considered.

To make the matter quite clear, let a practical illustration be taken. Let it be supposed that a house has to be lighted by a hundred incandescent lamps, each requiring a current of .75 of an ampere urged by an electromotive force of 100 volts. The rate at which energy is expended in each lamp, expressed in volt-amperes or watts, of which 746 were equal to a horse-power, will be $.75 \times 100$, that is 75. The energy expended in the 100 lamps will be at the rate of 7,500 watts, which are equal to 10.05 horse-power. But this, it must be remembered, is the actual rate at which energy is expended in the lamps. The energy that has to be developed by the engine is greater, for no dynamo-electric machine is perfectly efficient, no dynamo machine gives out as electrical energy the exact equivalent of the mechanical energy expended upon it. Let it be supposed that the machine used in our installation has a "commercial" efficiency of 80 per cent., that is, that 80 per cent. of the mechanical energy put into the machine reappears in the external or lamp circuit as electrical energy, the balance being wasted in heating the armature coils, and the friction of axles, slipping of belts, and other mechanical sources of loss. Then the rate at which energy is generated by the steam-engine must be $10.05 + 1.25$, that is 12.55 horse-power. This mechanical energy is to be produced by the combustion of coal, and if all the heat liberated in the combustion of the coal could be collected and utilized, the supply of coal required to generate energy at the rate of 12.55 horse-power would be very small; but, unfortunately, steam-engines,

even of the best make, have but low efficiency, and a horse-power-hour of energy requires in practice somewhere about 4½ lbs. of coal for its production; 12.55 horse-power-hours will therefore require about 56½ lbs. of coal—say, roughly, half a-hundredweight, the cost of which is not more than 6d. Assuming that the lamps were required to burn for 1,800 hours a year—that is on an average nearly five hours a day—the annual cost for coal would be £45. The prime cost of a suitable dynamo-machine and engine (with boiler) would be, say, £300, the interest on which, at 4 per cent., would be £12, and the annual depreciation, at 10 per cent., £30; the cost of attendance would be about £60, so that the prime cost would be £300, and the total annual cost £147, or £1 9s. 5d. per lamp.

If a galvanic battery is used to supply the current to the lamps, the energy will require to be produced at a rate of somewhere between 10.05 and 20.1 horse-power, for the efficiency of a galvanic battery depends upon the relations subsisting between the resistance of the battery itself and the resistance of the external circuit. When the greater resistance is reduced until it exactly equals the smaller, the greatest possible current is obtained from the battery, but exactly one-half of the energy liberated is wasted in heating the battery. When the external resistance is high, as compared with the internal, the current is less, but the percentage loss of energy is also less. As a matter of practice, therefore, it becomes advisable to arrange the resistances so that the external is greater than the internal, whereby the efficiency of the latter is made to as nearly approach a hundred per cent. as we wish. Let it be supposed that in our installation the efficiency is, as in the dynamo just considered, 80 per cent. To liberate a horse-power-hour of energy, a quantity of zinc must be oxidized equal to 2.022 lbs. divided by the electromotive force of the galvanic couple in use. It is calculable that the highest electromotive force obtainable in any zinc sulphuric acid battery is 2.248 volts, from which, however, a deduction must be made on account of the energy absorbed in the chemical action of depolarization. The amount of the deduction will depend upon the nature of the depolarizer: with nitrate of soda and sulphuric acid it is .708 volt, with bichromate of potash it is .343 volt, and with fuming nitric acid .284 volt. In order to do fullest justice to batteries, let it be assumed that strong nitric acid is the depolarizer used in our installation. The deduction to be made from the total electromotive force of the battery is, therefore, .284 volt, and the net electromotive of the battery is 1.964 volt, which corresponds to a consumption of 1.03 lb. of zinc per horse-power-hour of energy. To liberate 12.55 horse-power-hours, 12.93 lbs.—say 13 lbs.—of zinc must be used. If the price of zinc be taken at 2½d. per lb., the zinc used per hour costs 2s. 8½d. But the expense does not end here. Air, the oxidant of coal, costs nothing; whereas sulphuric acid, the usual oxidant of zinc, is of considerable value, costing, even on the commercial scale, ¾d. per lb.

To oxidize a pound of zinc, 1½ lb. of sulphuric acid is required, as is shown by the equation, $Zn + H_2SO_4 = ZnSO_4 + H_2$. To oxidize 13 lbs. of zinc, 19½ lbs. of acid are therefore required, which costs 1s. 2⅝d.; this, added to the cost of the zinc, makes about 3s. 11d. But we cannot stop even here. In order that the zinc may give the greatest possible return of energy, the electromotive force of the voltaic arrangement must, as has been stated, be high, and this—as a net result, at least—can only be obtained by the aid of a good depolarizing liquid. We assumed ourselves to be using strong nitric acid, and if we suppose that the average effect of nascent hydrogen upon this substance is to produce nitric oxide (N_2O_4), the amount of acid required will be shown by the equation $2\,HNO_3 + 3H_2$ (equivalent to $3\,Zn) = N_2O_4 + 4\,H_2O$. It follows, therefore, that the solution of every pound of Zn is accompanied by the deoxidation of ⅔ lb. of nitric acid. In the production of 12.55 horse-power-hours of energy, 8⅔ lbs. of nitric acid will therefore be deoxidized, the cost of which, at 6d. per lb., is 4s 4d. This, added to the cost of zinc and sulphuric acid, gives 8s. 3d. as the total cost of materials.

But we have not yet done. A battery cannot be charged, and then kept in action until every grain of zinc and every drop of acid have been consumed. It has been found by experience that no more than about 20 per cent. of the acid can

be utilized. When this proportion has been used, the battery begins to flag, and is soon of no further use for electric lighting until the solution has been changed. The greater part of the acid is thus removed as spent solution. But neglecting this source of waste for the moment, and assuming that by a careful process of "refreshing" it is possible to completely exhaust the acid solution, let us see what the cost of our 10.05 horse-power is when generated by a galvanic battery. We have seen that the cost of materials per hour is 8s. 3d., and assuming, as before, that the light will be required for 1,800 hours a year, the annual cost for materials only is £742 10s. 0d. The prime cost of the battery may be taken at £120, the interest, at 4 per cent., on which is, say, £5, and the annual depreciation, at 10 per cent., is £12. The cost of attendance is very difficult to determine. Primary batteries have as yet been used for electric lighting to such a small extent, that there are, practically, no records of experience whereon to base an opinion. It is claimed by the inventors of batteries that the cost is very small; they say that no skilled attendance is required, and this seems reasonable; and that there is nothing further to do than might be done by a domestic servant in the ordinary course of his duties. Let us assume that this is so, and that the cost of attendance is so small as to be negligible.

If we divide the total annual cost (that is £759 10s.) by the number of lamps, the annual cost per lamp is found to be £7 11s. 8d., as against £1 9s. 5d., when a dynamo machine is used. The difference is great, but it becomes even greater when we remember that it is never possible to completely use the acid solution.

From what has been said, it will be gathered that, in a comparatively large installation such as has been sketched, primary batteries have little chance of success; but for smaller installations, of, say, ten to twenty lamps, there may, perhaps, be a field of action open to them. Few people occupying a house requiring so few lamps as this would incur the trouble of a dynamo and engine, although for health's sake they might not object to the cost of the light as produced by a battery.

In what way is it possible to reduce the cost of electric lighting by galvanic batteries? It is evident that there are four ways open; and invention has been active in all. In the first place, a cheaper oxidizable substance than zinc may be employed; and in the second, possibly a cheaper oxidant than sulphuric acid. Next, contrivances may be adopted by which the solutions are more completely exhausted; and lastly, the bye-products —the substances formed during the action of battery—may be utilized in the arts.

In the first place, as to the oxidizable substance:—Zinc is almost universally employed, but lead and iron have been suggested, and a battery using the latter substance is now before the public.

Next, as to the oxidant, or, speaking more generally, the solvent of the positive element of the cell:—Sulpuric acid has been replaced, among other substances, by hydrochloric acid, and the battery employing this liquid has been introduced by Mr. Ross.

As to the bye-products, little can be said, except that the less said the better. Some promoters of patent batteries make a great point of the value of the bye-products of their batteries, but, as a matter of fact, their statements on this point will seldom bear investigation. A zinc residue contains a certain percentage of the metal in a state of chemical combination, from which it has to be recovered by a somewhat costly process, and one hardly likely to afford any profit to the person using the battery.

It may be interesting to notice that, in the Telegraph Department of the Post-office, no residues are thought worth preserving except the "black mud" (technically so-called) from the Daniell's, which contains a very large percentage of pure copper in the metallic state. Last year there were 69,323 Daniell cells in use, and the sum realized by the sale of the year's "black mud" was £167 14s., or rather more than a halfpenny per cell per year. An idea may be formed from these figures of the magnitude of the sum likely to result from the sale of the zinc residues—which in the Post-office are thrown away as worthless—in the case of an ordinary householder using some ten or twenty cells.

During the past few years, several bat-

teries especially designed for use in electric lighting have been devised; and the inventors of the more important have been good enough to bring their batteries here to-night, or to furnish descriptions of them. These batteries may be divided into two classes; those in which the liquids are kept in motion, and those in which they are allowed to remain in a state of quiescence. Of the latter, one of the most important is that devised by Messrs. Holmes and Burke. A description which these gentlemen have been good enough to furnish reads thus:—

"Each battery of eight cells can be charged or discharged at two operations, by means of a system of siphons formed partly in the substance of the outside cells. In order to charge the battery it is only necessary to fill a wooden measuring vessel with the exact quantity of fluid necessary to run into the battery, to connect this vessel with the long leg of one of the siphon systems, and to raise the vessel to a certain height, when the liquid immediately distributes itself amongst the cells, and is bound to attain a uniform height in each of them. To discharge the battery, it is only necessary to reverse the process.

"The fumes are collected and suppressed. The porous pots are sealed into wooden covers, which contain passages for the escape of fumes. The passages communicate with one main pipe formed in the substance of the cells, and the fumes can thence be led away to the fume box, where they are either dissolved or chemically absorbed, according to circumstances.

"The connections between the cells are so simple that they never can be coupled up wrongly, and they are placed beyond the reach of injury by acids or fumes. They never require disconnection when the battery is discharged or recharged. They require partial disconnection when the plates have to be amalgamated, but the latter, when once thoroughly amalgamated, hardly ever require re-touching. Plates have been run for two months without re-amalgamation.

"The depolarizing liquid is cheap and easily handled. It is made of nitrate of soda dissolved in solution of sulphuric acid of a particular strength. It depends for its action on the formation by the current of nitric acid in the porous pot. The hydrogen entering decomposes the nitrate of soda by the aid of the acid, forming sulphate of soda and nitric acid. The liquid is of very good conductivity, and as the nitric acid is only formed as it is wanted, the inconvenience of handling this very disagreeable acid is avoided.

"The cost of the liquid is only 5½d. to 6d. a gallon. Each gallon furnishes from 750 to 800 ampere-hours of electricity at an electromotive force of 1.92 volt—*i.e.*, about 1.536 watts, or very nearly two hour horse-power.

"The internal resistance of the battery is very low for a primary cell, and equals .02 ohm. as nearly as possible."

It may be added that, like the generality of electric lighting batteries, the batteries of Messrs. Holmes and Bourke employ carbon and zinc as the elements.

The battery which next claims attention is that of Mr. O. C. D. Ross, of which a model is on the table. The chief point of novelty distinguishing it is the ingenious arrangement for charging and discharging the depolarizing liquid.

The battery itself is enclosed in a box, about 1 ft. square by 7 ft. long, which contains thirteen double cells, the plates in each of which weigh about 10 lbs.; the whole box when charged weighs about 2 cwt. Each cell contains two carbon plates (formed of ½ in. carbon rods placed side by side) which are immersed in dilute hydrochloric acid mixed with one-sixth of its bulk of a mysterious compound, called by Mr. Ross "Eureka," but which might doubtless be replaced, without materially affecting the action or the cost of the battery, by nitric acid. The zinc plates dip into a solution of common salt, the liquids being kept apart by porous partitions. The mechanical arrangement by which the carbon cells are filled and emptied is very simple; a horizontal pipe connected by flexible tubes to the bottom of each inner cell is raised or lowered; one end is attached by a flexible tube to a cask of acid, and the other end is led to a waste tank. When the horizontal pipe is raised, and communication with the waste tank cut off, the acid flows, on turning the tap of the acid cask, into the inner cells. When spent liquid is required to be removed, the horizontal pipe is lowered, and the liquid flows from the cells to the waste tank. The outer cells are filled with solution of

salt, in one operation, by pouring the solution into a groove extending the whole length of the box, and connected by side passages with the cells.

The Ross battery has been examined by Dr. Hopkinson, who reports that:—

"Comparing this battery with others, a great deal may be said in its favor.

"Its constancy with a large current is remarkably good.

"The liquids used in the cells are less expensive than in any battery having any pretence to constancy.

"Mr. Ross's method of drawing off and replenishing the liquid is very convenient."

The electromotive force per cell is stated to be 1.82 volt, and the resistance 0.06 ohm.

Another battery, introduced some time back, is that known as the "Edeo" or "Heap" battery. It is a carbon-zinc couple using bichromate of potash as the depolarizer, and dilute sulphuric acid as the excitant. The cells are lined with lead, and hold each 1½ gallons of the bichromate mixture (which can be purchased ready prepared at 1s. per gallon). The zinc plates are 10 × 6 inches, and so offer a total surface of 120 square inches to the action of the acid; they weigh 3 lbs. each. The electromotive force is stated to be 2 volts per cell, and the resistance 0.2 ohm. The point of novelty is claimed to lie in the preparation of the bichromatic solution, the description of which is best given in the manufacturers' own words. They are as follows:—

"In each lead cell pour six pints of water, and in each repeat the following operation:

"Fill the colander (which is supplied with the battery), with bichromate of potash, and hang it in the cell so that the crystals are just immersed in the water. Pour about one-fourth of three pounds of sulphuric acid over the crystals in the colander slowly, stopping occasionally when the liquid boils. As fast as the bichromate of potash gets dissolved, add more, and continue pouring in sulphuric acid as before, until there are used up for each cell all of one and a-half pounds of bichromate of potash, and three pounds of sulphuric acid. When the liquid is entirely cool, it is ready for use. . . . This fluid will last twice as long as the . . . fluid ordinarily supplied."

Mr. Heap is good enough to exhibit a three-cell battery in action to-night.

In the other or circulating class of batteries there is one deserving of notice. It is the invention of Messrs. Oliphant, Burr, and Gowan. It is a two-fluid bichromate battery, having zincs coated with an extremely thin film of gold before amalgamation. The exciting liquid is a solution of salts of mercury. The liquids for the outer jars and inner porous pots are pumped out of separate tanks into distinct pipes communicating with the first jar and the first porous pot. They then rise up siphon pipes into the adjoining cell, and flow onwards in this way through the whole series. To avoid any possible stoppage through friction in the pipes, it is advisable to have the first cell half-an-inch higher than the second, and so on. Thus, in a series of six cells, the last cell will be three inches lower than the first. The last jar and porous pot are connected to their respective tanks. The tank holding the bichromate solution is fitted as a cell, and works a Griscom or other motor, which causes two small pumps to keep the liquids in constant circulation. The pumps raise two cubic inches of liquid per stroke, and work at the rate of thirty strokes per minute. The pumps are constantly at work day and night, whether the battery is in use or not.

The effect of gilding the zincs before amalgamation is, it is stated, to materially decrease local action.

Many other batteries might be described, and it may be interesting to note that, during the past three years, about 150 patents for improvements in primary batteries have been applied for in Great Britain alone.

A question which presents itself in connection with the problem of economical lighting by primary batteries is this: Is it not possible to effect economy in the lamps—to so improve the lamps that a given expenditure of energy on the part of the battery will develop more light in the lamps than is now obtained? Yes: it is possible. It would be troubling you too much to enter here into a lengthy account of the scientific principles which should guide the designer of the luminous conductors of incandescent lamps. It

must suffice to say, that the ideally perfect conductor would assume a spherical form; its mass would be very small, and its surface also, and the material of which it was composed would possess the power, possessed unfortunately by no known substance, of enduring without change for an unlimited time the passage of a very powerful electric current. Of all substances hitherto used for incandescing conductors, carbon obtained by the intense heating of a pure hydro-carbon is the best. It will give a greater returning light for a given expenditure of energy, and will withstand the action of the conductor longer than any other substance. It will become possible, therefore, by its aid to approach more nearly to the ideally perfect form of luminous conductor than we should otherwise be able to. If also we can so fashion the intimate structure of the carbon, that the particles already have that arrangement which they tend to assume under the action of the current, the type of perfection can be approached even more nearly. This has been done. In a patent obtained in November, 1882, by Messrs. Boullon and Soward, in conjunction with myself, a method was described by which luminous conductors can be obtained of pure deposited carbon formed under the influence of the electric current. By suitable means, these conductors can be obtained of any required degree of approximation to the theoretically perfect form. What thickness they will ultimately assume it is impossible yet to say. The shorter and finer they are made, the greater is the economy, but the shorter is their life. The thickness at which the economy in action is just balanced by the increased cost of renewal of lamps is the practical thickness, but this can only be determined by lengthy experiments, which are still in progress.

It may be noted in passing that high resistance luminous conductors—and they can be made without difficulty with a resistance of 1,000 ohms and upwards to the inch—have the great advantage that they work with a small current, although they necessarily require a somewhat high electromotive force. This enables the copper leads to be much thinner, and therefore less expensive, than in the case of a low resistance luminous conductor, worked by a large current, which is an important point worth bearing in mind. Messrs. Woodhouse and Rawson have quite recently introduced a new lamp, which they call their "hair filament lamp." These lamps have an extremely thin, hair-like filament, whence their name. No particulars of the method of manufacturing these filaments have been published, but it appears that, like the filaments of my colleagues and myself, they are made of deposited carbon. The method of their preparation is carefully kept secret, doubtless for fear of piratical imitations which, although generally easily detected, are yet often hard to prove in a court of law. These filaments are stated to be very efficient. It is said that they will furnish a given amount of light with less than half the expenditure of energy necessary to furnish the same amount in an ordinary thick filament. This is doubtless true: the experiments of my colleagues and myself with our own filaments perfectly support this statement, and we have had the opportunity of experimenting with filaments of much greater length, and therefore light-giving power, than Messrs. Woodhouse and Rawson seem to have had, if a conclusion may be formed from the lamps before the public.

THE RECENT DANISH ARMOR PLATE TRIALS.—Rarely have experimental tests of armor plates been more closely watched, or followed with greater interest, than those which have lately been concluded at Amager, near Copenhagen. The principal reason for the attention thus exhibited will, no doubt, be found in the circumstance that, whereas most naval powers have definitely settled the question as to the system of armor to be adopted, the Danish Admiralty decided not to be governed by foreign official reports, but rather to judge for themselves respecting the merits of the only principles of armor plate manufacture at present in use, viz., the French steel plates and the English compound plates. The question has now been decided in favor of Wilson's compound armor, and the order for the armor plates for the Ivar Hvitfeldt has been placed with Messrs. Cammell & Co., of Sheffield. This solution of the question will not surprise those who have followed the various stages of the trials as reported in these columns. Yet it must be acknowledged that the Danish Naval Commission have had to contend with unusual difficulties in arriving at a conclusion, for it was known from the first that the Amager trials, instituted as they were, on an independent basis, would have important results. It is a source of gratification to us to know that the order has been placed with an English firm.—*Iron.*

THE METROLOGY OF THE GREAT PYRAMID.

"Who is this that *darkeneth counsel* by words *without knowledge?*
Gird now thy loins like a man, for I will demand of thee, and answer thou me.
Where wast THOU when the foundations of the PYRAMID were laid?
Declare if thou hast understanding.
Who hath *laid the measures of its base*, if thou knowest?—Or who hath *lifted the plumb line against it?*
Whereupon are its *socket stones made to sink?*—or who hath laid upon it the *headstone thereof?*"
Paraphrase of Job xxxviii. 4, 6.

BY LIEUT. C. A. L. TOTTEN., U. S. Army.

Written for VAN NOSTRAND'S MAGAZINE.

The opponents of the theory, that the Great Pyramid of Gizeh was built as an intentional exponent of cosmic truths, have lately had occasion to hail with a certain degree of satisfaction the advent of a new literary effort to put their case upon a more defensible position. In a reprint from the *School of Mines Quarterly*, entitled, "The Imaginary Metrological System of the Great Pyramid of Gizeh,"* President Barnard, of Columbia College, has renewed the attack upon the Pyramid, somewhat after the manner of Professor Proctor, and somewhat upon a line which he considers new, and hence deems to be a theory of his own. Taking, however, a similar stand to that originally occupied by Prof. Proctor, the greater part of President Barnard's rhetoric is expended in ridicule and denial.

But mere ridicule is a dangerous weapon. While, indeed it is barbed and pointed, its unbalanced shaft is so utterly devoid of fledging that the archer can never be certain what will be the point of fall to its erratic trajectory. The half-made arrow may return upon the very one who gives the wing to its uncertain flight. Nor is mere denial a potent weapon of defence in an argument of such magnitude as the one which now surrounds the subject of the Great Pyramid. Not only is it an unscientific method, for it will not turn a standing beam, but it is beneath the dignity of those who enter into a public debate upon a scientific subject. We broadly claim that the unsupported employment of these two weapons—ridicule and denial—in the discussion of a subject of such magnitude as that which deals with the origin and object of the Great Pyramid, is equivalent to relying upon a pointless stick as a weapon of offence, and upon a paper circus hoop as shield against *riposte*. The argument has long ago been lifted from the dust of such an arena, and demands the skill of trained and well-equipped contestants.

That the subject is really one of magnitude, of scientific import, and of vast and growing interest, is borne out sufficiently by the fact, that it has begotten such a controversy as it has, that such a host of opponents are hurrying into print, and that now, not a year passes in which each side of the controversy does not swell its teeming library. It is not a little strange, too, that the mystery of the subject increases under the penmanship of every one who writes a book upon it. Prof. Proctor's hostility to what he opprobriously termed the "Religious Theory," led him to the attack. Convinced at last of the weakness of mere ridicule, and awakened to much of the undoubted scientific teaching of the monument, he was forced to offer some explanation for facts monumentalized therein too boldly to be simply the results of accident and coincidence. Hence he propounded a theory of his own, and named it an "Astrologic Theory." The fact is, one cannot devote himself to this topic without at last being convinced of the certainty of Promethean design in all its lines and proportions. Hence, if unwilling to adopt theories already advanced, or if committed, as President Barnard undoubtedly is, in being also the President of the French Metrical Bureau, to a new-fangled metrology, to which the Pyramid theory is inimical, the only resort for those who come to the topic, and at length become absorbed by it, is to propound a new theory of their own. This,

* Reviewed briefly in the August No., p. 174.

as just remarked, is exactly the case of President Barnard. Unwilling to accept openly, even the purely scientific theory of the monument—a theory which Petrie himself does not dispute—he states that if the Pyramid is really "a π Pyramid, as it undoubtedly is, it is so *by accident.*" So, too, throughout his discussion of the monument, every cosmic fact or ratio which we meet is declared to be an accident. Nevertheless, waxing warm at length in his subject, even the President of the Metric Bureau cannot resist the tendency to theorize, even if it only be in ridicule, and so he propounds a "Lunar theory connected with the worship of Isis."

Unfortunately for our estimate of President Barnard's information upon pyramid matters, he proposes this theory sarcastically, and in utter ignorance of the fact that the whole ground he endeavors to ridicule has long ago been covered in sober earnest by Joseph Baxendell, F. R. A.. S. Mr. Baxendell, an able astronomer, and a firm believer in the ideas of Prof. Smyth, long ago noticed all of the lunar references of the Great Pyramid alluded to by President Barnard as his own discoveries, and they have been familiar matters for years to all well-informed pyramid students. Indeed President Barnard might have found these lunar relations set forth at length in Mr. Proctor's volume of the Pyramid's solution, a volume which he certainly has no excuse for not having consulted.

The President does not, however, propose his theory in good faith, but in a spirit of sarcasm and innuendo. He puts it forth merely as *moonshine* "for the lunatics," who are bathed in pyramid fantasies, to walk in. He says he does so in the magnanimous hope that in thus ridiculing the whole subject he may at length "induce some of his fellowmen to apply a little common sense to the study of a subject which has been heretofore involved in an elaborate web of ingeniously contrived mysticism."

We do not intend to review this effort of President Barnard at any great length, simply because to answer all of his headings would require a volume much larger than his own. Life itself is far too short to be spent in attempting to set right every pedestrian, who is recklessly astray upon a road where careful surveyors have already erected numberless and sufficient guide posts.

Those familiar with the dignity and scope of this topic cannot but arise from the perusal of President Barnard's paper with a feeling of dissatisfaction that, after all, he has introduced into the "Battle of the Standards" no new forces, and in reality has failed to rally even those already put to flight. The whole argument of *design* based upon the exquisite harmony of the monument, a harmony which pervades and connects each chamber and passage with every other, has no weight whatever with President Barnard. It has either escaped him entirely, or else, having no explanation for coincidence, so consummate as to be mathematically beyond the realm of chance, he has purposely avoided its discussion.

But this harmony is the prime motor of the mental process by which the mind of a candid man is always compelled to recognize design and continuity. Once convinced of the nature of the facts actually built into this monument, one might as well doubt it as an architectural reality, as to question the cosmic intentions of its builder in its due and intimately related proportions. Did the evolution theory of the School of Science, to which President Barnard belongs, rest upon facts as clearly made out as are those built by human hands into this wonderfully proportioned building, there would be no defensible ground for its rejection. But the very scholars who are loath to accept even human intention in the planning of the Gizeh monument, argue devotedly in favor of the evolution theory, based upon premises of the same kind, though infinitely less convincing. Their ways are thus not equal. Even Mr. Petrie and Mr. Proctor are forced to admit that an unique *system* of structure runs throughout this monument, while it is only in a spirit of sarcasm that President Barnard ever does so.

In his discussion of the subject each claim of the believers in cosmic design receives a separate examination, and at the doctor's hands is made to appear somewhat out of gear, or to be slightly at variance with modern scientific estimates of the things monumentalized. It is as if one denied the existence of a chain, or claimed that its continuity was

a matter of mere chance, because its several links were somewhat worn, or bent with age, and with misuse.

To those who may have an opportunity, and who are willing to examine both sides of this matter for themselves, it will be apparent that President Barnard has by no means weakened the chain of evidence in thus pointing out, here and there, a link misshaped. For besides the argument of *continuity*, which he does not at all dispute, nor even allude to, there always remains the fact that modern science itself does not certainly know the exact value of any cosmic truth, by means of which President Barnard can pretend to pronounce unerring judgment upon the absolute accuracy of the Pyramid. Nor will the fair mind, bent upon reaching the *truth* of this monument, ever lose sight of the still more astonishing fact that every new and modern calculation of these cosmic data results in a closer approximation to those actually built 4,000 years ago into the monument.

Throughout his book, President Barnard loses no opportunity of frequently reiterating the statement that these ancient peoples could have had no means of determining these facts by actual calculation. He also denies that they knew or even used them in their daily life. To have possessed them would have made them "modern" in every sense of the word, and to have had them and not have used them is preposterous. This, too, is exactly what the Pyramid students themselves claim. It is their fundamental premise; it is, in fact, the very one of all others upon which the conclusion that the architect wrought wiser than he knew, was inspired—must have been inspired—is firmly founded. For the facts are there, harmoniously disposed, and always occupying middle ground between the extremes of the best modern estimates, and as these estimates converge, upon recalculation they always have the pyramid values round about their mean. Moreover, this pyramid value is always found to be a true "mathematical function," dependent upon the "problem of the three bodies," and resulting from its proper discussion. They were not known as facts of practical value, or of cosmic import to the world in the day which saw the pyramid erected. The very π value is only of modern discovery, and could not have been known to the ancients, yet it is laid out architecturally an hundred times throughout the monument. Was this chance? The modern doctrine of chances replies to us that the odds against a successive coincidence so astounding are in the ratio of one hundred raised to the hundredth power to one. This is unity followed by 102 ciphers, a number past conception, indeed it is a number which is upon the very verge of our utmost powers of enumeration. And yet the π values and harmonies of this monument are few in number compared with the myriad of others linked to them, that reflect and multiply throughout the structure the fundamental facts of every human science known.

Let us examine, for instance, a single one of the links which President Barnard claims is bent, and which he therefore would have us believe is not a link, and hence that the chain of evidence is not a chain at all. We can examine any one of them, but as modern science has perhaps studied the Sun Distance with greater care, and at far greater expense than any other question we will select it.

President Barnard argues upon the Pyramid's reference to this distance, as follows:

"The article of the Pyramid faith which presents itself for examination next in order is the proposition that the height of the Pyramid is exactly one one thousand millionth part of the mean distance of the earth from the sun. Inasmuch as we do not know the exact mean distance of the earth from the sun, it is impossible to subject this statement to the test of a rigorous comparison; but inasmuch as the builders of the Pyramid knew in all probability a great deal less about the matter than we do, it is next to impossible that the proposition should be true. Supposing it, however, for the sake of argument, to *be* true, the fact may be accounted for on either of two hypotheses—it is true accidentally, or it is true intentionally. Against the first of these hypotheses, the probabilities are in the proportion of infinity to zero; and this alternative, moreover, would be rejected with scorn by the disciples of the pyramid religion. But that the fact may be true *in the intention of the builders*, as

asserted, we must assume for them a knowledge which we have no reason to suppose them to have possessed. This slight difficulty the true believers dispose of summarily, by taking it for granted that their human ignorance was illuminated by direct inspiration from on high. The orthodox doctrine on the subject, therefore, is that the true distance of the sun was miraculously made known to the Pyramid architects, and therefore that the height of the structure itself was made, not pretty nearly but, exactly the one-thousand millionth part of that distance.

Now what are the facts on which this extraordinary assertion rests? Simply these: In 1867, Mr. William Petrie, having satisfied himself that the perimeter of the base of the pyramid 'has been *proved* to symbolize a year, or the earth's annual revolution around the sun,' and, further, 'that the radius of that typical circle had also been shown to be the ancient vertical height of the Great Pyramid, the most important and unique line which can be drawn within the whole edifice,' was moved by these weighty considerations to conclude that the same line '*must* represent also *the radius of the earth's mean orbit round the sun:* and in the peoportion of 1 : 10⁹, : or 1 : 1,000,000,000: because, among other reasons, 10 : 9 is practically the shape of the Great Pyramid. For this building' he proceeds, 'notwithstanding, or rather by virtue of, its angle at the *sides*, has practically and necessarily such another angle at the *corners*, that for every *ten* units its structure advances inward on the diagonal of the base, it practically rises upward, or *points to sunshine* by *nine*. Nine, too, out of the ten characteristic parts (viz., five angles and five sides) being the number of those parts which the sun shines on, in such a shaped pyramid in such a latitude near the equator, out of a high sky.' (*Our inheritance in the Great Pyramid*, Edition of 1874, p. 48.)

This is the logic, and the whole of the logic, on the basis of which a fact is asserted which, to plain human reason and simple common sense, is so grotesquely improbable, that its possibility can only be defended by clothing it with the character of miracle. The true believers have not failed, accordingly, to be affected by a sort of painful misgiving lest this *a priori* argument should be found unsatisfactory to a skeptical world. They have therefore sought for a confirmation of their hypothesis in comparing the result furnished by it with those deduced from the most recent investigations of the solar parallax.

Since early in this century the mean distance of the sun has been generally taken to be about 95,000,000 miles. This value has been obtained from the parallax as computed by Encke at Altona, in 1822, with data derived from the transit of Venus of 1769; a parallax which he found to be 8″.5776, giving for distance 95,454,000 British statute miles. In 1857, Prof. Airy proposed a new method for determining planetary distances, founded on displacements of Mars in right ascension, as observed morning and evening from a single station, when in opposition at its nearest approach to the earth. This method was applied in 1862, by observations made at Victoria, New South Wales, and by others made at the Royal Observatory, Greenwich, during the opposition of Mars in that year; and the results, as computed at Greenwich, gave a parallax of 8″.932, and a distance of 91,551,000 miles. In the meantime, Leverrier at Paris announced (in 1861) that in order to reconcile discrepancies in the theories of Venus, the earth and Mars, it was necessary to assume a parallax approaching perhaps 8″.95. From this is obtained a solar distance of 91,357,000 miles. Chambers (Astronomy p. 3) assumes as a probable value of the parallax 8″.94, which gives 91,465,000 as the distance. On the other hand, Prof. Newcomb, in his Popular Astronomy says: 'It would appear that the solar parallax must lie between pretty narrow limits, probably between 8″.81 and 8″.86; and that the distance of the sun in miles lies between 92,200,000 and 92,700,000 miles.' Furthermore, Prof. Young in his recent book on the sun, remarks: 'It would seem that the solar parallax cannot differ much from 8″.80, though it may be as much as 0″.02 greater or smaller; this would correspond to a distance of 92,885,000 miles.'

Besides the astronomical methods, of which there are others not here noticed, there is one of great scientific interest dependent on the velocity of light. The time required for a luminous impulse to

reach the earth from the sun is known within about a second or less, so that if we can ascertain how far light travels in a single second, we are possessed of all the data necessary to secure a very close determination of the distance of the sun. Independent methods for the experimental solution of this problem were devised some years ago by Messrs. Fizeau and Foucault, of Paris; and one of the other of these methods has been repeated by M. Cornu in France, and by Lieut. Michelson of the U. S. Navy Academy at Annapolis. The results obtained by the experimenter last named, are regarded as very closely approaching the exact truth. They give 299,900 kilometers as the velocity of light per second; and as light is 498 seconds in coming to us from the sun, we find the solar distance to be 149,350,000 kilometers, equal to 92,803,464, miles. This nearly corresponds to a parallax of 8″.81 which would give a distance of 92,876,000 miles.

Now the distance of the sun as worked out on the Pyramid theory is 91,837,000 miles. This is in the neighborhood of some of the astronomical determinations. It exceeds the lowest of them, and falls short of the highest. Prof. Smyth accordingly jumps, in this case as in many others in which a favorite quantity of his falls somewhere among several of the same class, directly to the conclusion that because his quantity is neither the smallest nor the largest, therefore it is the true one. But if we consider the judgments of astronomers as to these various determinations, we shall see that the inferior ones have no standing with them at all. Newcomb, our highest authority in this country upon physical astronomy, places the probable distance at about 92,500,000 miles, and Young puts it as high as 92,885,000; while the experiments on the velocity of light, which are esteemed to furnish the result most worthy of confidence, give a result almost identical with the estimate of Prof. Young. The Pyramid distance is, therefore, only about a million of miles too short; whi'e, probably, the uncertainty which still attaches to scientific deductions is not so great as half a million.

But there is another consideration which is quite conclusive as to the question now under discussion. The pyramid is said to be, and appears to be really, a π pyramid. But in a π pyramid the height is equal to the perimeter of the base divided by 2π. In such a pyramid, when the base is given, the height is unalterably fixed, and cannot have a value assigned to it at the caprice of the constructor. Now it is a doctrine vital to the pyramid religion that the side of the base shall contain the sacred cubit of the Hebrews as many times exactly as there are days, integral and fractional, in the tropical year. The height must, therefore, be that which this supposition demands, and it can be neither more nor less. On the other hand, if the height had been arbitrarily fixed at one one-thousand millionth part of the earth's mean distance from the sun, the side of the base must have had a value such as *this* hypothesis requires; and could not have been determined by the cubit and the number of days in the year. It exceeds all the bounds of credibility, it exceeds all the powers of credulity, to suppose that two such entirely independent and inconsistent schemes of construction should prove to be harmonious in their practical results. Such a coincidence is indeed physically possible, but the chance in favor of the possibility is less than one to some millions of infinities."

Thus reasons President Barnard. The question for discussion is therefore the Sun Distance, as monumentalized by the grand gnomon which stands upon the center and the border of the land of Ham.

It has been claimed by Pyramid students, for 30 years, that the vertical height of this monument bears a direct reference to this sun distance, that the circumference described by this height as a radius is not only equal to the square perimeter of the fundamental base, but that the length of this circumference or the square base contains as many times 100 *axial* inches, (*i. e.*, 500 millionths of the earth's polar axis) as there are *days* in the year. If this is so, then it is not only a *proof* of cosmic design, but one that is most appropriately built into the very parts of the monument, where, by the relations of the three things typified, and the form of the Pyramid itself, it naturally belongs.

If, however, President Barnard's sarcastic conclusion is correct, our link is more than bent; it is broken, and the chain of evidence a disconnected

one. But we unhesitatingly reply that this conclusion is absolutely erroneous, and we propose to show it upon his own ground. Let us quote it again and then we will be better able to discuss it. It is as follows:

"The Pyramid distance is therefore only about a million of miles too short; while probably the uncertainty which still attaches to scientific deductions is not so great as half a million."

Several things are here to be noted. In the first place there is the broad admission that even modern science rises from its most carefully studied problem, and from one upon which all the modern civilized nations have repeatedly united, sparing no expense, still doubtful by perhaps half a million of miles. This best modern deduction places the probable distance at about 92,500,000 British miles, with a probable increment of some thousands, if Professor Young's experiments upon light are fundamentally correct. We shall therefore accept this "best modern estimate" as to the sun's distance, convinced that a problem so faithfully and expensively studied, cannot have been solved with an error that shall ever hereafter cause the result to vary very considerably from 92,500,000 miles.

What now is the Pyramid sun distance? President Barnard is seriously in error in making it out to be the height multiplied by 10 raised to the 9th power. He quotes, it is true, Prof. Smyth as his authority, but Prof. Smyth *qualifies* this statement in that he expressly says that 9 on 10 is only "*practically*" the shape of the Great Pyramid. That is, not absolutely; 9 on 10 is an *approximation*. It is a wonderfully close and practical approximation, too, and it is one that is nearer the absolute truth than modern science is certain of the distance under discussion, by President Barnard's own admission. For the 9 on 10 theory of the slope places the sun distance at 91,840,011 miles, while that arrived at by modern science after all of its labors is really doubtful after its very first figures.

Now if the Pyramid is a π—Pyramid, as President Barnard admits "it undoubtedly is," it cannot be an exact 9 on 10 Pyramid. The question is therefore to determine *exactly*—I speak mathematically—what is the slope of the monument up the arris or hip lines, as they climb, from the fundamental, southeast, socket up towards the vertex of the cap-stone?

This slope is accurately expressed by the ratio.

$$\text{Slope of a } \pi\text{–Pyramid} = \frac{20\sqrt{2}}{\pi} \text{ on } 10$$

or carrying the calculations out to 14 places of figures, it is 9.0031631615711+ on 10.

This slope is so very close to the far more simple one, expressed in whole numbers, to wit; 9 on 10, that in common parlance the latter is generally used instead of the more cumbersome though absolutely exact decimal ratio. Either President Barnard knew this, or he did not. To say that he knew it would make him out as unfair in argument and scientific investigation, as upon page 22 he implies that Prof. Smyth has been. To say he did not know it is to point out that he has failed to comprehend the nature of the very question he is, with ex-cathedral vehemence, endeavoring to criticise.

Lest it may here be objected that the defining of a pyramid by its slope up the arris line, rather than directly up its face, is a fanciful method, and one without authority, it is to be noticed, that, as the latest measurer, Mr. Petrie, admits, "the designs of the various slopes that are met with, appear to be always a simple relation of the vertical and horizontal distance." It is in this very way that the Egyptian hieroglyphics themselves define them. For Ahmes, in his mathematical papyrus, defines pyramids by their "*sloping height up the arris edge, and their diagonal of the base beneath that line*" (Petrie): So that the natural and *Egyptian* method of stating a *slope*—a question which bears so strongly on the whole planning of the pyramid—is to say that it is built by the

$$\frac{20\sqrt{2}}{\pi} \text{ on } 10, \text{ template.}$$

This, it will be noticed, increases the 9 on 10 approximation, by somewhat more than the .003th power, and gives us a function without error, and one to which President Barnard dare not advance an objection. With this expression for the slope the Pyramid sun distance (S) therefore becomes

$S = H(10)^{\frac{20\sqrt{2}}{\pi}}$, in which H is the height of the Pyramid.

Mr. Petrie is the latest measurer at Gizeh, and a disbeliever in most of the Pyramid theories. But, so far from overturning the cosmic theory of the Great Pyramid, as enunciated by Prof. Smyth, Mr. Flinders Petrie's work has magnificently endorsed it. His measurements of this building are throughout so nearly identical with those of Prof. Smyth that they prove the entire honesty of the latter, and fully corroborate all the ratios which he and Taylor originally noted. It was the elder Petrie, his father, who first advanced this sun distance reference. It is not a little strange, therefore, that the son by his measurements should unwittingly and yet so thoroughly support his father's theory, in which he does not believe. The very valuable work which details Petrie's labors at Gizeh should be in the hands of every one now engaged upon the solution of this engrossing problem. Especially should the opponents of the cosmic theory read and study it more closely than their own writings show they do. Now the careful measurements of Mr. Petrie enable us to determine the ancient height of the Pyramid from the floor level of the southeast or lowest socket, to the summit of its very capstone. He gives the data as follows:

From vertex to pavement level=5776″.0 ± 7″.0
" pavement to s e. socket level= 39 .9

∴ Petrie's height over all =5815 .9 ± 7″.0

That is, it is practically 5816″. with a possible seven inches of *doubt* in either the positive or negative direction.*

Now if we add less than one half of the margin of error, actually here allowed by Mr. Petrie, to his own height of this ancient monument, we shall obtain the very height always claimed for the monument by maintainers of the cosmic theory. The height claimed by them is 5818″.62+ British inches, which is equal to 5813.01 ± Pyramid inches. Each of the latter is .001±, greater than the present British inch, and is an even 500 millionth of the earth's polar axis.

Expressed accurately by a circular and circummetric function, and for such a π–Pyramid this height is

$$H = \frac{180.\,A}{\sqrt{\pi}} = \frac{32400''}{\pi\sqrt{\pi}} = 5818''.622870+$$

in which A is the "analytical unit" (57.29°+)

Let us now treat this height, which from its possibility of exact mathematical expression is absolutely without error, by the equally absolute arris slope of the Pyramid just determined. The function for the sun distance will then be accurately as follows:

$$S = H(10)^{\frac{20\sqrt{2}}{\pi}} = \frac{32400''}{\pi\sqrt{\pi}}(10)^{\frac{20\sqrt{2}}{\pi}}$$

$$= (5818''.622870+)\,(10)^{9.0031631615711+}$$

but $(10)^{9.0031631615711+}$ equals the number whose common modern logarithm is

9.0031631615711+

since the logarithm of 10 is 1. Thence taking the common logarithm of H and adding it to this logarithm we shall have the logarithm of the required height in inches.

Thus,

Log. H= 3.764820201265401358181 07
" $(10)^{\frac{20\sqrt{2}}{\pi}}$ = 9.0031631615711+

Log. S= 12.7679833628365
∴ S= 5861157.....″
= 92,505,634..* miles!

This remarkable result, which tallies so closely with Prof. Newcomb's estimated "probable sun distance" (92,500,000.± miles), as quoted so sarcastically by President Barnard against the mistaken 9 on 10 approximation, *is calculated to hammer at least one link of the chain of evidence into perfect shape upon the anvil of every candid judgment.* Nor is there a single one of the points of real pyramidal significance, which have thus again

* Mr. Petrie's measure has thus admittedly a possible error of ±7″.0. Upon page 15 of the introduction he says: "The probable error of all important measurements is stated with the sign ± prefixed to it as usual" . . . And further on he says, "and I will only add a short definition of it as follows: The probable error is an amount on each side of the stated mean, within the limits of which there is as much chance of the truth lying as beyond it."

With these statements, which show the accuracy with which Mr. Petrie accomplished and records his splendid measurements, we are ready to continue our examination of the subject.

* The number corresponding to the logarithm of the result has only been calculated to the seventh figure of the inches, because of the difficulty attending the process. It is calculated far enough however to avoid error greater than unity in the *miles.*

been disputed and put into controversy by President Barnard that will not yield to the sledge-hammer of mathematical truth, in this same manner!

It is apparent that the above argument would not be affected in the least by the most extreme refinement of computation. For an error of 200 feet in the earth's polar semi-axis represents 1000 miles or so in the sun's distance, and an uncertainty of a second in the parallax involves about 100,000 miles therein. The pyramid pronounces in favor of a solar parallax of $5''\sqrt{\pi} = 8.''86226925+$, a number which is ten times the well-known "constant" used for obtaining the equality of squares and circles. This is exhaustively shown by Mr. Chas. Latimer, C. E., in the March (1883) number of the *International Standard*, where he also points out the fact that the base of the Pyramid

$$=100\,\frac{81''}{\frac{1}{2}\sqrt{\pi}}=\frac{8100''}{.886226925}=9139''.8712581$$

Now it is a remarkable fact, perhaps more remarkable than any we have yet considered, that the height by which we have just determined the true sun distance, *demands* for the perimeter of the square indicated by the level of the south-east, or fundamental socket of the pyramid, a measure of exactly

$4\times(9139''.8712581+)=36559.''4850324$

(Brit. in.)—a distance which reduced to pyramidal inches, or to 500 millionths of the polar axis, is the year number $(365.2422)\times 100$.

This number, $9139''.8712581+$, has always been claimed by modern "pyramid students" as the actual fundamental base side indicated by the Pyramid. It is now proved to be so by Mr. Petrie's splendid measurements, and is shown above to be a true circular and circummetric function. The latest measurer at Gizeh, Mr. Flinders Petrie, claims, however, that the true base of the Pyramid is but $9068.8''$, in which connection it has been lately remarked; "it had been a fundamental article in the Pyramid religion that the base should measure just 9140 British inches, and as it proved to measure only 9068.8, that with this determination, the beautiful union of the *sacred* cubit (25.") and the length of the tropical year (365.242) melts away into thin air."

But this by no means follows when we get at the true facts of the case. Mr. Petrie's base (9068.8 inches) is that of the Pyramid measured horizontally along the upper surface of the *pavement* which surrounded it. It is not that of the fundamental square indicated by its lowest or south-east *socket* level. For *reasons of his own*, Mr. Petrie has *assumed* the pavement base as the real one to be studied. It was no doubt set out with most beautiful skill, and with a manifest object as to length. But it is the *fundamental* square that pyramid students have always maintained as of tropical year significance, and the *total* height of the monument from this fundamental level up to the vertex of its capstone as of solar distance import.

Upon page 38 of his "Pyramids and Temples of Gizeh," Mr. Petrie admits that "the sockets were cut to receive the sloping face, which was continued right down to their floors *beneath the pavement*." . . . "Hence the sockets *show the size of the Pyramid where it was started from varying levels, which were all under the pavement.*"

Mr. Petrie further gives as the measure of the sloping line from the south-east socket up to the north-east socket, the length $9130.8'' \pm (?)$. Pyramid students find that rigid mathematics demand $9131''$-$.05+$—a difference of less than $.3''$! Now 365.242×25 (that is the year number by that (25) of the "sacred cubit:") $=9131''$-$.05$. Hence this sloping line from the fundamental socket to the one next to it, expresses the required cosmic ratio in pure and simple figures. As it is a *sloping* line its mathematical significance is that of *ratio*, a fact which should not be lost sight of in this discussion.

Again, the sloping line being $9131''.05$, the *horizontal* one from the south-east socket to the arris line produced through the north-east socket—*i. e.*, the base side of the *fundamental* square—is exactly $9139''.87+$ inches, or is the functional value demanded by pyramid students, *and thus again proved by Mr. Petrie's measure.*

Against the possibility of such a coincidence President Barnard claims, in the closing paragraph of his argument, that the chances are more than "*some millions of infinities to one.*" Yet here it is President Barnard, along-side of the true sun distance, linked to the true solar parallax,

and linked to each, in a cosmic monument, by the actual and modern π value, which was unknown and unused by the ancients. The question therefore for the candid and unbiassed man to answer, is, whether he will stultify himself at the mandate of such investigators as have yet arrayed themselves against the cosmic theory, or admit the monument to be a wonder past the ken of human intellect.

While I am willing to admit that there is much mistaken enthusiasm displayed by pyramid students in their engrossing study, it is nevertheless a fact that the study of this monument is opening up the whole topic of Metrology in a way that sets it before the world as the veritable science of sciences. Its students now include some of the most earnest men of both Church and State, and their investigations have long since raised the "Solution of the Pyramid Problem" far above the discussion of mere coincidence.

Mathematically speaking, there are no such things as "coincidences." In this rigid science "what is must be," and the deeper we examine the Great Pyramid the more our conviction grows that it is a consummate exponent of universal science, or *Metrology*.

Mr. Barnard finally concludes that while it is "indeed physically possible, that two," and we might add that *several*, "such entirely independent and inconsistent schemes of construction should prove to be harmonious in their practical results," still it would "exceed all the bounds of credibility, and exceed all the powers of credulity" if they should prove so. *But they do prove so*, hence as facts they are credible, hence too they being facts so physically beyond the range of accident, our credulity must cease, and in its place give room to honest conviction of magnificent design and, if it be not possible that such should be of human origin, then we must yield to our convictions and admit the overruling guidance of a hand that wrought far wiser at the Pyramid than the architect himself. If President Barnard and his school decline to accept this alternative, and we challenge them to dispute it, then they must gorge themselves with "several millions of infinities" in the presence of all right-minded men, and in a state of torpor digest their unwholesome and dyspeptic morsel.

To what purpose therefore shall we further discuss phases of this controversy that have long since been worn out. If the opponents of the cosmic theory have any *new facts*, to relate, which militate against the premises we base our faith upon, we ask them in the interests of science to show them forth. All searchers after truth desire to know them, but until they have, it is far better not to dip their pens in simple ridicule and innuendo, and attempt to wreck a Pyramid, against whose huge proportions few minds can measure themselves with any great advantage.

THE DEFECTS OF STEAM BOILERS AND THEIR REMEDY.

By Mr. A. C. ENGERT.

From "Iron."

Steam boilers are used as a medium to convey heat, or the accumulated force produced thereby, to an engine for the purpose of moving machinery to do certain work. The heat rises from the burning fuel within the boiler furnace, and penetrates the metal to heat the water in the boiler to 212° F. so as to form steam; but before its actual formation 966° F. has to be absorbed. Steam has no power to move a piston at such a low temperature, therefore more heat is required to produce the necessary force, and the higher the degree of heat contained in the water and steam, the more pressure will the steam exert on the piston when allowed to enter the cylinder. It is of the utmost importance to have perfect combustion, so that the whole of the heat may enter the water in the boiler. For this purpose various shapes and forms of boilers have been invented, but the Cornish and Lancashire types are found the most convenient for general purposes. Now 1 lbs. of good coal is capable of evaporating 15 lbs. of water.

but these boilers are only capable of evaporating one-third or at the best one-half of the quantity of water they should do in proportion to the fuel consumed (which is far too small a proportion), owing to the flame and heat wave rushing through the flues too quickly for the heat to thoroughly penetrate the metal. Improvements have, therefore, been introduced, such as Galloway and other cross tubes, in the Cornish and Lancashire boilers, which have proved to be very advantageous in saving fuel, so long as they are kept clean. But even with these improvements the average evaporation is not much more than half that which theory shows it should be owing to incrustation. This evil of incrustation, together with unequal expansion and contraction, and the corrosion in boilers, are hindrances which ought to be removed. In the author's opinion several of these evils are due to the construction of Cornish and Lancashire boilers. For instance, the ashpit, which is used to convey air to the burning fuel in the furnace, allows cold air to enter which keeps the bottom part of the boiler cool, and stays the circulation of the water within. If warm water even be fed into the boiler a part of the heat is again lost from the above cause, and the heat supplied to the bottom of the boiler is also partly lost, so that the lower part of the boiler is almost entirely useless for forming steam. Another trouble arising out of this evil is the unequal expansion of some of the plates. The top part of the boiler being hot and the bottom cool, causes great strain at various seams, as the expansion and contraction are continually affected by the varied draught or temperature, often loosening rivets and causing leakage, and endangering life and property. Another bad feature in this description of boiler is that the bottom of the flue and the shell being very close together it is hardly possible to clean those parts properly. This is of great importance, as on that particular part the most settlement takes place, the water being there almost stationary.

Among the general evils to which boilers are subject, incrustation is one of the principal, because all waters contain certain impurities which settle in the boiler, as the heat cannot dissolve nor drive them off with the steam. These settlements remain generally where the steam is formed, and get baked into a hard scale on the plates, but mostly on the crown of the flues and conical tubes. As this scale is a non-conductor of heat, it requires 15 per cent. more coal when the scale is only one-sixteenth of an inch thick; and when it is a quarter of an inch thick, 60 per cent. more coal is required. It is not only the coal which is thereby wasted, but the water is kept away from the boiler-plates, which become overheated, and eventually have holes burnt in them. To prevent incrustation, various solutions and many other remedies have been tried. The author desires to draw attention to the remedy which the chairman of the Manchester Steam Users' Association has recommended. This consists in putting 3 lbs. of soda ash every day in the feed water of a 50 horse-power boiler, and to frequently blow off. Besides this, he recommends the Porter-Clarke process, which consists in putting lime water into the feed water, which throws down all the carbonates, and the water is then filtered and ready to be pumped into the boiler. Still, this process does not touch the sulphates.

Another trouble with boilers is the formation of smoke and the loss of unconsumed gases. In reference to this, the author had the honor, in June 1881, to read a paper before this society "On the Prevention of Smoke"; he also sent a letter to the editor of *Iron* (which was published in that paper for December 21, 1883) entitled "The Cause of Smoke," of which the public has never been informed. A few points therein may, perhaps, be restated here, as some of the younger members of the profession may have thought the subject too unimportant to be enquired into. The author wrote as follows: "When fresh coals are placed on the fire in an open fire grate smoke rises directly. The cause of this smoke is not very far to seek, as it will be easily understood that before the fresh coals were put upon the fire within the grate, the glowing coals radiated their heat and warmed the air above, and thereby enabled the rising gases at once to combine with the warmed air to produce combustion, but since the fresh coals are placed upon the fire these coals absorb

the heat, and the air above remains cold. The fact is that these volatile rising gases cannot combine with the cold air for combustion, still a combination does take place in the following way: The cold air in the act of combination absorbs a part of the warmth of the rising gases which they cannot spare, and therefore they (the gases) must condense, so much so that small particles are formed, which aggregate, and are called smoke, and when collected form soot; but so long as these particles or gases are floating they cannot burn or produce combustion. Of course the invisible hydrogen gas rising from the coals with the smoke would burn easily enough, but it must be first lit before it can burn." The above sentences only refer to open fire grates; in furnaces smoke and waste of gases may be caused by too small a quantity of air, so that there is not enough oxygen to satisfy the rising gases, or there may be too much air, which, whilst passing through the furnace, absorbs heat and thereby impoverishes the combustion. In the paper on the prevention of smoke, read before this society, the author expressed a strong opinion that smoke once formed could not be consumed, and gave his reason, and he finds the following in a work frequently consulted by boiler makers: "We see, then, how palpably erroneous is the idea that smoke once formed can be consumed in the same furnace in which it is generated, and how irreconcilable is such a result with the operation of nature, and it is only the cooling down of carbon which produces smoke." Here then again smoke is attributed to the cooling down of the gases, and the author thinks the cause not only of the loss of a few grains of carbon and the other invisible gases (which amount often to a high percentage) which go out with the smoke, but also the trouble and unhealthiness produced by the smoke cannot be referred to too often. It is, of course, well understood that cold air must be prevented entering the furnace, and for this purpose mechanical stokers have been invented, not only to prevent the formation of smoke, but to save fuel. Experience, however, has taught us otherwise. Referring to the Manchester Smoke Abatement Exhibition, *Engineering* of April 28, 1882, after stating what the various mechanical stokers profess to do, observes that very many mechanical stokers are thrown out of use after a time, and the old hand-firing again resorted to. It is also remarked that though the machines are pleasing and satisfactory at first, the main result in the long run is that the mechanism is troublesome, wasteful and costly. Later on it is stated that "it would be interesting to know the actual number that has been fitted to boilers during the past five years, and in how many cases they are still at work." In the report of the Manchester Steam Users' Association, April 24, 1883, is the following: "The question of smoke prevention, and the relative merits of mechanical and hand firing, had been carefully considered by the association, and the conclusion arrived at was that, as regards economy, the two modes of firing gave practically the same result; as many as fifty-five mechanical stokers had been removed from boilers under inspection. To prevent smoke by hand firing, nothing out of common was wanted, but only a reasonably fair draught, a reasonably fair boiler, regular firing, and the admission of a little air above the firebars to secure the combustion of the gases." In the report of the Jurors' Committee of the Smoke Abatement Exhibition, it is stated on page 25, as to mechanical stokers, that "most of them require occasional assistance by hand," thus showing that they are still open to further improvement. In the above report it is stated, on page 27 (Firebars, fire-bridges, furnace-doors, &c.), that "the committee are of opinion that any assistance by which air is introduced with the view to complete the combustion either of gaseous or of solid fuel is objectionable, and is not likely to lead to the most economical results."

These statements of high authorities support the author's opinion that smoke cannot be burned in the same furnace where it has been formed, and much less is any furnace door able to produce complete combustion of the fuel burning within the furnace. In fact, entire combustion is almost beyond the means of man, in an ordinary furnace. Still, improvements have been made by introducing warm air, to combine better with the rising gases of the burning fuel. But even this is no easy matter, considering

that 150 cubic feet, or 22 lbs., of air are required for each pound of coal burnt; and it is a fact that the carbon requires great heat before it can be burnt, and further, that all solid fuel formed into gases and compressed gases while expanding absorb a large quantity of heat before they can burn. On this subject, *Engineering*, of December 2, 1881, says: "Total combustion means, not merely the rendering the products of combustion invisible, it means the conversion of the carbonic oxide produced into carbonic acid." Another author says: "The carbon burning in the flame has not completed its combustion, and it is only the high temperature of hydrogen burning which will enable the carbon to combine more readily with the oxygen in the air." It is only the clear white flame which will show that combustion is in its proper condition and everything is well heated. To prove that the carbon in the flame is only completing its combustion may be seen by the flame of a gas jet or of a candle, which gives a very white light, as, for instance, the Siemens gas lamp. But if a little air be allowed to blow in from the bottom, then the white light will blaze out in a long dark red flame, and give little light, but plenty of smoke and soot. Again, if a plate be held over a gas or a candle flame, carbon, *i. e.*, soot, will settle on it, and the deposit is increased when the plate is held lower in the flame, in fact the burning of carbon in a flame is only one of the stages of combustion, and when combustion is interrupted or incomplete it produces smoke, which is increased, while the invisible steam formed by the hydrogen will condense and cling to the dust of the carbon. This can be proved by the flame in a furnace; the lowest part of the flame where the carbon has just combined with the oxygen is generally much darker, even when the top is quite white, and thereby gives the greatest heat of some 2000° F., and when the flame is entering the tubes of a marine boiler, the bottom tubes are getting choked with soot, and the top flues, owing to the white flame, remain clean, and produce the heat required for forcing steam. When fresh coals are placed in the furnace, the stoker generally leaves a small quantity of fire on the dead plate upon which he places his green coal, and then closes the furnace door. This method the author thinks entirely wrong, as the heat from the fire underneath drives out the gases from the green coal in such rapid succession, that it is almost impossible for combustion to take place, even if the materials were ready for combustion. Such, however, not being the case, most of these valuable gases escape and are unproductive. If the fire on the dead plate were raked to the front near the door, and the green coal placed on the dead plate between the two fires, the small quantity of air entering the furnace door when shut would be warmed, and would ignite the rising gases from the green coal. At any rate, the first gas rising from the green coal is generally hydrogen, and as this gas has a greater affinity for oxygen than for carbon, it takes the first supply to form invisible steam and producing the greatest amount of heat whilst burning, thereby preparing all the other gases for proper combustion. To carry out this plan, the dead plate might be made a little larger so as to receive more coal at longer intervals, but the carbon gas at first very seldom combines with more than eight volumes of oxygen, forming carbonic oxide, as the carbon gases have to be formed from the solid carbon, and only afterwards while burning combine with eight more volumes of oxygen to form acid. Of course, if the draught is slow, which is always better for proper combustion, and if there be more carbon gas within the flame, then another six volumes of carbon gas is absorbed, and oxide is formed again. This then would be something like proper combustion, and would produce the heat required. But where is the quantity of warmed air to come from in the Cornish and Lancashire boilers? As the jurors state, in their opinion "air ought not to be allowed to enter above the firebars." Some very elaborate furnaces with hot air chambers have been built, but they are so expensive, and occupy so much space, that they are rarely employed. The atmosphere passing through the firebars, and incandescent fire above, is not at all times sufficient for complete combustion, especially when the draught is weak; and as it has been shown that burning carbon is taken up by another eight volumes of oxygen, it is almost necessary to apply

warm air above the firebars if the proper combustion of all the gases is to take place. The densest smoke is formed when stoking is commenced, or the partly burned coals in front are pushed backwards on the firebars. The coals now disturbed having been warmed and their pores opened, the previously confined gases rise in a mass, but meeting the cold air entering partly from the open fire-door and partly through the firebars, the gases condense and form smoke. To save this great loss of gases, and to prevent the formation of smoke, the damper ought to be nearly closed before the furnace doors are opened. If this plan is not adhered to, the owner of the boiler will not only lose all the gases as stated, but in addition some 8 lbs. to 10 lbs. of coals, to make good the cold which has entered the furnace and flues by the door.

Priming is another trouble common to boilers in general, and although a very important matter, the author has been unable to find any reference to this subject even in the most important works on boilers. He, however, found in a paper read before this society by Mr. Wm. Major, on April 9, 1877, the following: "It is certainly humiliating for the profession to be obliged to admit that one hundred years after the introduction of the steam engine they are still unable to subdue an evil so generally experienced as priming of steam boilers; yet such is the case, as proved by the *Serapis*." Further on it is stated that tallow and other remedies have been tried with various results, some doing harm to the boiler plates. It is, however, stated that petroleum, mixed with the feed water according to Mr. Major's invention, proved perfectly successful, as shown by a letter from Copenhagen, about one gallon of petroleum being used per day in a 30 horse-power boiler. Beyond these statements very little is said about the cause and remedies of priming. The author will now give his opinion and experience of priming in boilers. The warming up and starting of an engine in the morning requires a large amount of steam, as considerable condensation takes place at first, more especially in the winter. The great quantity of steam taken from the boiler in so short a period very often reduces the pressure of steam within the boiler some 25 to 40 per cent., and considering that the flues by that time are not very hot, and are not able to form steam, the whole formation of the steam required is thrown on one small plate or space just over the bridge of the Cornish or Lancashire boiler. In fact, this small part of the boiler has produced or formed the whole of the steam and pressure within the boiler, and therefore this part must be very hot, and the water close to it absorbs more heat for expansion to form steam, and is consequently lighter than the water just above it, and possesses greater accumulated heat for the moment than the steam bubbles already formed. The heated water, with the lighter steam bubbles, are therefore thrown up together, when the pressure from above is suddenly removed, and forced into the engine. The working of a vertical boiler may serve to make this clearer. The steam formed having to force its way up through a column of water from $1\frac{1}{2}$ to 3 feet deep, of course produces some pressure, and the water on the top being cooler than that at the bottom, the single bubbles of steam are hardly able to force their way up, and a combined effort is required (which may be said to cause a convulsion within the column of water) for the steam to raise itself, and then the superheated water is thrown up with the steam. This so-called priming in the vertical boiler is not seen in the water gauge so long as the water is kept low, say about 3 inches in the glass, but if the water be pumped as high as, say, 5 or 6 inches, then the water thrown up with the steam shows itself in the glass from the top. This, then, proves that the water forced up with steam will by gravity fall back in about a space of 3 or 4 inches from above the water line, and the steam so cleared will show very little priming. Pure water will, no doubt, prevent priming to a great extent, but where boilers are forced or the water stands too high in the boiler, then the convulsion will be stronger, and a higher space for the steam to clear itself will be required. These facts prove two things—firstly, that in most instances the heat is applied on too small a space, and thereby harm is done to the boiler; and, secondly, that the steam space above the water line is insufficient. These two conditions not only destroy or do harm to many boilers

and engines, but cause a loss of fuel and power. The best means for the prevention of priming will be explained later on.

The author thinks too little attention has been given to the air and gases with acids contained in the feed water by steam users in general, as these acids not only corrode the boiler plates and produce galvanic action with the grease returning to the boiler with the condensed steam, but the gases accumulating in the boiler amount to something more than is generally supposed. All water contains at least 2 to 3 per cent. of absorbed air and gases; such air and gases being continually pumped into the boiler accumulate. It is known that all gases are by gravity heavier than steam, the steam being on the top in the boiler is allowed to pass from thence to the engine, the gases remaining behind, and, as carbonic acid gas is the heaviest gas, it takes the lowest place. Now, a 50 horse-power boiler will use about 3,000 gallons of water per day; then there would be as much as 14½ cubic feet of various gases accumulated, and in a week some 80 cubic feet. These gases can only be expelled from the boiler by frequent blowing off; and if this be neglected, the gases and acids accumulate to a very great extent; and the author is of opinion that if an explosion then took place, it would be much more violent on account of the accumulated gases. They increase in volume as 1 in 490 at every degree F., so that at 50 lbs. pressure the accumulated volume of gases will nearly be doubled, and thereby give an after pressure to the steam to blow the boiler to atoms. The Manchester Steam Users' Association and other authorities have expressed the opinion that there must be something beyond steam pressure in many explosions. The return side and bottom flues of a boiler ought to be rounded off, as all corners prevent the proper flow of the air and heat waves. A few bricks sticking out in the flues from the brick walls will throw the heat-wave towards the boiler.

The author having stated some of the existing faults and their remedies in Cornish and Lancashire boilers, will now explain the improvements made in a boiler designed by himself, which has undergone some very severe trials. The object of the author was principally to do away with the ashpit; to prevent, if possible, incrustation; to extract from the water all gases; to prevent unequal expansion; to produce dry steam, and, by having better combustion, to prevent smoke. To obtain these results, he designed and had manufactured a boiler* in which two flat flues are placed one above the other in a cylindrical shell 16 feet 6 inches long by 7 feet diameter, of which the top flue is 4 feet 8 inches wide by 10 inches high, and the other 4 feet 6 inches wide by 10 inches high, the top one being stayed with 24 and the bottom one with 33 3½-inch vertical tubes screwed into the fire-plates and expanded. These two flues have one fifth more room for water and steam than the two flues of a Lancashire boiler, the top flue having its front bottom plate slanting downwards to make room for the firebars and furnace. On the front of the boiler a cast-iron box is fixed with the bottom open, having doors in front with slots and slides and perforated baffle plates. The firebars rest in front on a strong bar within the cast-iron box, and the back end of the firebars rest on the top end of the slanting flue. The front ends of the firebars are turned up to prevent the fire falling out. A sheet-iron curtain hangs down, so that when the doors are opened the fire is not exposed; when firing is required, the curtain is raised, and the damper is nearly closed automatically, so as to prevent cold air rushing into the furnace and flues. On the back end of the boiler there is a closed tank, 4 feet 8 inches wide, extreme height 4 feet, and 1 foot 10 inches deep. The water is supplied by gravity from a higher cistern, and a 2-inch pipe is carried from the tank through the roof, for the purpose of discharging the air and acids. The firebars sloping towards the front were designed so that the air should not merely pass through the firebars, and then rise to the boiler plate, but that the burning gases, drawn towards the flue, should continually meet and amalgamate with the other gases and meet the warm air coming through the slots in the front doors, so as to produce carbonic acid close under the boiler-plates, and to evolve the greatest heat at the proper places. The flame or heat waves in their

* This boiler is described and illustrated in *Iron*, vol. xxii., page 309.

passage are retarded by the vertical tubes, which they embrace, and thereby spread the heat to the top and bottom plates; the other part of the flame rushing by will do the same duty by the next tube, and so on to the end of the top flue. Here the heat wave is obstructed by a water tank for boiling the water contained within it, and the same process goes on at the bottom flue. Passing out at the front, the heat is led to the right side flue, returning under the bottom of the boiler, and passing to the left side towards the damper and shaft. The water does not remain long enough in the tank to be sufficiently boiled to throw down the carbonates and sulphates of lime, and therefore a live steampipe is carried from the boiler into the tank, and enters a large 4-inch pipe, which, with 50 lbs. pressure, will produce close to the pipe some 240° to 250° F., which heat will throw down the carbonates and sulphates (which are drawn off below) and send the acids and air out of the tank. The feed-water supply is taken from the hottest place over the pipe and is pumped into the boiler, and as this water is almost pure, and at the boiling point, it produces no settlement or scale within the boiler.

The steam supplied to the water tank is then led by a pipe to the front under the firebars, and allowed to escape in small jets for the purpose of warming the air under the grate. The steam passing through the incandescent fire will be partly dissolved into its two elements, oxygen and hydrogen, of which the oxygen will at first fly to the carbon, and the hydrogen will burn above with fresh oxygen again to invisible steam, which, if collected, would produce nearly a $\frac{1}{2}$ ton weight of water from a ton of coals, if proper combustion had taken place. The steam jets are principally for the purpose of obviating the necessity of a very strong draught. The damper being half closed to prevent the heat from entering the chimney, the quantity of steam supplied under the front bars is very small, for if a large supply of steam was used it would become superheated by the burning fuel, and rise by its gravity close under the boiler plate, thereby preventing the heat from penetrating the metal, and proving that steam ought never to be employed above the firebars. The vertical tubes after one year and a quarter of work are found to be as clean as if they had only just been put in. The circulation is so rapid there is no settlement on the bottom of the boiler. On the top flue only there is found a small quantity of sediment, which is easily removed by a water hose, and the scum on the sides of the boiler can be easily brushed off. Another advantage is that the bottom of the flues produce almost as much steam as the top, because no stationary steam globules are permitted to accumulate in consequence of the rapid circulation through the vertical tubes, in which no settlement or scale can attach itself. However large the flat flues are made, it is impossible for them to collapse on account of these vertical tubes or stays. The total heating surface is 577.7 square feet, and the grate surface 13 square feet. There are, therefore, 44.4 square feet of heating surface per square foot of grate surface. The author finds, that by a slow combustion and a gentle draught, more heat is evolved and penetrates the metal, and that therefore more water is evaporated per lb. of coal.

There being no ashpit in this boiler, and the boiler being entirely bricked in, no unequal expansion can possibly take place. As no cold air is allowed to enter the furnace, no smoke can arise except at the moment the curtain is let down and the damper raised; but this vapor is so trifling that it is hardly worth mentioning. A boiler on this principle was laid down by the author for driving the machinery at his works in Three Mills Lane, Bromley-by-Bow, in December, 1882, and has been tested by several independent engineers, amongst others by Mr. W. H. Maw and Mr. D. K. Clark. With this boiler there is no priming, dry steam being produced (as stated in *Engineering* for September 14, 1883), tests having proved it to be thoroughly dry. Mr. D. K. Clark, in his report, states: "I tested the steam produced each day for dryness; it proved to be dry and free from priming water." The explanation of the dryness of the steam is very simple, and is due to the large flat surface of water in the boiler, of about some 112 square feet area, which for the most part is only 3 inches deep, and having a heated flue-plate of some 80 square feet below,

and twenty-four vertical tubes, which with a volcanic action continually send up the steam formed within them, and that from below, caused by widely-distributed heat waves. Then there is such a rapid circulation that no pressure arises from a volume of water, and there is no force or convulsion present to form steam bubbles. With shallow water and well-distributed heat, priming is therefore prevented.

Thus the principal defects of boilers have been remedied by this new design. There is very little waste or ashes, no clinkers in general, and only a few cinders, which may be used again. The stoking is very simple, as the coals are placed almost in front, the bars are short, and slanting to the front. In *Iron* of October 5, 1883, it is stated by the editor that this boiler has realized the highest efficiency on record, and that it marks a new phase in boiler construction. In the *Times* of September 17, 1883, the editor favored the invention with a similar opinion. Mr. W. H. Maw and his staff made five tests during nearly six months, of which the author here gives the last. An elaborate report of the same was published in *Engineering* of September 14, 1883, in which it was stated that the boiler embodies a number of novel features, and produces rapid circulation through the vertical tubes, the effect of the curtain being very marked, and that as a rule there was no trace of smoke.

RESULTS OF EVAPORATION TESTS, Aug. 31, 1883.

Duration 8. A. M. till 5 P. M., 9 hours.
Steam pressure, maximum, 50 lbs. per square inch.
Steam pressure, minimum, 39.5 per square inch.
Steam, mean, 45.5 per square inch.
Coal, total quantity burned, 1,015 lbs.
Coal, quantity burned per hour, 112.8 lbs.
Coal, quantity burned per square foot of grate (short bars), 8.68 lbs.
Ashes weighed, containing fragments of coal and clinker for which no allowance was made, 39 lbs.
Area of grate, 13 square feet.
Water, total quantity evaporated, 1,150 gallons.
Water evaporated per hour, 127.7 gallons.
Water evaporated per square foot of heating surface, 2.2 lbs.
Water evaporated per lb. of coal, 11.3 lbs.
Temperature of feed water, mean, 81.6° F.
Temperature of gases measured in flue between damper and chimney, 290° F.
Draught, mean, in inches of water, $\frac{5}{16}$ inch.

The National Smoke Abatement Association had two tests made by their engineer, Mr. D. K. Clark, who gave a minute report of the boiler in question, and from which the following figures are taken. The report is dated January 18, 1884.

RESULT OF EVAPORATIVE PERFORMANCE AND EFFICIENCY OF THE ENGERT STEAM BOILER, LONDON, 1883.

Number of Trial.	1.	2.
Date of trial	Dec. 20, 1883.	Dec. 21, 1883.
Temperature of boiler-house, average	59.3° F.	57.3° F.
Barometer Average	30.03 inches.	29.90 inches.
Duration of trial	8 hours.	8 hours.
Average effective pressure per square inch in the boiler	41.6 lbs.	36.6 lbs.
Net quantity of coal consumed at trial	1,064 lbs.	1,568 lbs.
Net quantity of coal consumed per hour	133 lbs.	196 lbs.
Net quantity of coal consumed per hour per sq. ft. of firegrate.	10.23 lbs.	15.10 lbs.
Ash (no clinker) residue	74 lbs.	84 lbs.
Ash residue per cent. of net coal consumed	7 per cent.	5.36 per cent.
Average temperature of water supplied for consumption	73.5° F.	70° F.
Quantity of water supplied for consumption	202.5 cubic feet.	236 cubic feet.
Quantity of water per hour	25.31 cubic feet.	29.5 cubic feet.
Quantity of water per hour per square foot of grate	1.95 cubic feet.	2.27 cubic feet.
Quantity of water per pound of coal consumed	11.85 lbs.	9.37 lbs.
Quantity of water per pound from and at 212° F.	14.18 lbs.	10.96 lbs.
Quantity of water per pound of combustible	12.73 lbs.	9.90 lbs.
Quantity of water per pound of combustible from and at 212° F.	14.90 lbs.	11.58 lbs.

Mr. Clark concludes his report with the following remarks:—"On the first day the boiler was worked at the usual pace to supply steam for daily requirements; on the second day it was forced by increasing the draught, in order to test still further the capabilities of the system. In point of efficiency the results

compare favorably for the Engert boiler. On the system of firing for this boiler, which does not demand more than an ordinary degree of care in management, smoke can be entirely suppressed. Shortly after the two days' tests were completed, the boiler was opened in my presence, and I found on inspection that it was entirely free from scale, and was otherwise in good order. I may add, in conclusion, that in my opinion Mr. Engert has produced a steam boiler of much merit, and that he has made a new and effective departure in steam boiler engineering."

The interior of the boiler has also been inspected upon two occasions by the engineer of the London Mutual Boiler Insurance Company, the last inspection having been made on November 9, 1883. Upon each occasion the inspector reported the boiler to be clean and free from scale or deposit.

In conclusion, the author would submit that the defects of steam boilers and the evils of incrustation, priming, and bad circulation are largely, if not wholly, due to faulty construction, and that he has succeeded in remedying these defects. He submits this with confidence, not so much upon his own authority and experience as upon those of independent engineers who have investigated the working of his boiler, and have reported favorably upon it.

THE FOUNDATIONS OF THE NEW CAPITOL AT ALBANY.

By WILLIAM JARVIS McALPINE, M. Inst. C. E.

From Selected Papers of the Institution of Civil Engineers.

The Author has not met with sufficient information to establish a rule for determining the load which may be safely imposed by a structure upon earth of a specific character and condition. The degrees of consistency and compactness in different kinds of earth and their mixtures, and, above all, the extent of moisture therein, so affect the supporting power as to discourage any attempt at a formula for practical use. The nearly universal rule seems to be, to depend upon the previous experience of the locality, or upon observations of structures supported on similar earths in like conditions; in fact, to guess at, what the Author believes may, in most cases, be determined with considerable precision, and avoid on the one hand the unnecessarily costly foundations which are so frequently observed, and, on the other hand, those inappropriate and insufficient foundations which cause the destruction of the superstructure.

He was requested ten years ago, to devise plans for the foundations of a large and costly State building, which had to rest upon soil apparently equable and stable, but which proved, on careful examination, to lack these qualities to a remarkable extent. It was also found that the earth, to a great depth under many portions of the foundations, received and parted with a considerable amount of moisture with the changing seasons. The circumstances of the case did not allow the use of piles or inverted arches; it was, therefore, necessary to spread the base of the walls over such an area as would afford the requisite sustaining power, and also to protect the clay and sand from any excess or deprivation of its natural degree of moisture, so as at all times to derive from it the same degree of support. The importance of the work warranted the expense of experiments to determine the questions above referred to. In the absence of any similar or equally extensive experiments, the author is induced to submit the present ones, in the hope that an explanation of the methods adopted, and the results attained, will prove serviceable.

The structure, though a single building, may be considered as a collection of a dozen large ones, with great differences of elevations, and weights upon the lower walls, and yet so bonded together as to require that the pressure of each of the parts should be the same per square foot on the earth beneath. This object has been fully accomplished; and when the

structure is loaded to the maximum extent of 200,000 American tons, the author believes that it will not compress the earth upon which it rests more than three-fourths of an inch, and exactly the same under every part thereof. The building measures nearly 300 feet by 400 feet on plan, and has three main stories and a basement. The lower walls are 110 feet high, but those of the corner towers, pavilions, and main tower, are of much greater height.

The ground covered by the structure sloped eastward at the rate of 1 in 25. The pit was excavated to a depth of 5 feet below the natural surface at the south-east corner, and 25 feet at the north-west corner. The excavation, together with the borings which were made in the bottom of the pit, fully exhibited the character of the earth. The lower strata (termed in the locality "blue clay," and "Albany clay") are more than 100 feet in thickness, resting upon the Hudson River Argillite (a clay state), the two forming the banks of the river for 30 miles of its course. The "blue clay" contains from 60 to 90 per cent. of alumina, the remainder is fine silicious sand. It also contains many nodules of clay, highly charged with carbonate of lime in the form of rings and discs about an inch in diameter. Overlying the blue clay was a mass of earth from 1 foot to 35 feet deep, composed of the same clay mixed with sand of different degrees of fineness, in proportions varying to such an extent as, when saturated, to render it in some places, a semi-fluid, while in others it was nearly pure sand, and very porous. This material occurred in veins and strata, large and small, above and below the level fixed for the foundation. One of the largest of these veins of viscid earth passed diagonally across the foundation, and at a depth of 6 to 20 feet below the bottom of the pit. It was 200 feet long, and from 5 to 25 feet wide.* Other veins and strata of less size, were found extending across the bottom, and sometimes terminating in pockets in the blue clay. Borings, from 10 to 30 feet deep, were made in several places below the bottom of the pit, which showed the substratum to be blue clay; and a well which had been sunk close by, to a depth of 100 feet, was entirely in the blue clay.

The earth in its natural condition at midsummer contained from 27 to 43 per cent. of moisture. When the samples were thoroughly dried and pulverized and again fully saturated (without dripping), they absorbed from 39 to 46 per cent. of water. The blue clay ordinarily held about 40 per cent., and when dried, again absorbed about 43 per cent. It was, therefore, as a rule, completely saturated in its natural state. It was upon this kind of earth that the subsequent experiments of the supporting power of the clay were made. The pure clay, obtained by separating it from the sand, weighed 116 lbs., and the sand so separated 80 lbs., per cubic foot; but, when they were again mixed in different proportions the weight of the mixture was less than the proportionate means between them. Earth taken from the same places as the samples, varied from 81.5 to 101.4 lbs. per cubic foot, depending upon the proportions of the clay and sand; and these weights show, to some extent, the relative supporting power of the earth at the places from which the samples were taken.

It was originally intended to support the structure upon wooden piles, of which a considerable number had been procured before the author was entrusted with the direction of the work. Many comparatively large buildings in Albany have been supported upon wooden piles driven into the blue clay or upon thick planks laid under the walls. In a few cases the wood used for this purpose has been found in tolerable preservation* half a century after it had been buried in the blue clay; but, generally, such timber was much decayed at the end of a quarter of a century; and several heavy buildings, after having stood firm for twenty years, began to settle, and the walls to crack, in consequence of the decay of the wooden supports and the unequal settlements therefrom. It appears that when the clay had been kept constantly moist, the wood did not materially decay in half a century; but, wherever the moisture was drawn off, the wood did not last more than twelve years. In this case, even if

* This vein was dug out, and replaced with clay and sand artificially mixed, moistened, and slightly rammed in layers, so as to render it as similar to the adjacent natural material as possible.

* In digging the trenches for the street pipes of the new waterworks at Albany, the author had occasion to remove many of the old pine water-pipe logs, of which only the sap wood was decayed. They had been buried in the blue clay for more than half a century.

a wooden foundation could have been arranged so as to be kept constantly wet, it would have ultimately decayed; and its use was, therefore, inadmissable. Cast-iron piles of white iron could be relied upon for a century or more, but would also have eventually decayed.

The use of sand and concrete piles, made by boring or driving holes into the clay and filling them with these materials, was also considered. For reasons which will subsequently appear, inverted arches could only be used under a part of the structure,* and it was deemed advisable to have but one system of support. The author, therefore, finally determined upon the plan which has been executed.

In most buildings, except where spires or towers are introduced, the weight is nearly equably imposed upon the several foundation walls; but in the Capitol the main and pavilion towers are much higher and heavier than the adjacent walls. The extremely heavy fire-proof floors, loaded as they will be frequently with dense crowds of people, books, &c., must necessarily carry their load to two only of the four surrounding walls, and, with some of the roofs acting in the same manner, will produce very unequal pressures upon the foundations.

The weight of the whole building and its contents when in use will be 200,000 American tons. The area of the base of the exterior and court walls, and the rear walls opposite, is about 24,000 square feet, and sustains an average of 6½ tons per square foot on the basement walls. The main tower, which weighs 30,000 tons, has an area of 2,508 square feet, equal to 12 tons per square foot upon its foundation walls. The weight on the foundation under the exterior walls of the corner towers is 47 tons per lineal foot; on the interior walls of the same towers, it is only 39 tons; and on the adjacent division walls, 23½ tons. Still greater differences in the weight on adjacent walls occur in other parts of the building, especially at the main tower, where the weight is 134 tons per lineal foot, and on the adjacent walls but 47 tons and 39 tons.

* It was necessary to arrange to carry two-thirds of the weight upon the exterior, rear, and court walls, which are separated 120 feet on two of the fronts, and only 90 feet on the other two. Inverted arches spanning three very unequal spaces would have imposed unequal loads upon the clay beneath, and their use would have defeated the design of distributing exactly the same weight upon every part of the clay beneath the structure.

Passing around the exterior walls of one quarter of the structure (the remainder being a repetition of the same sized walls), the weights to be supported per lineal foot are successively as follows: commencing at the main tower, 134 tons (which may possibly be increased); the adjacent walls are 47 tons per lineal foot for 60 feet; next, 44½ tons for 60 feet; next, 47 tons for 120 feet (turning the corner tower); next 44½ tons for 60 feet; next, 67 tons for 18 feet; and next, 50 tons for 52 feet to the center of the south or north front. On the rear of each of these walls, the interior wall is loaded with 39 tons, and the division walls with 8½ to 23½ tons per lineal foot.

The exterior walls of cut granite facing, backed with rubble and brick, average 150 lbs. per cubic foot. The floors, including the iron box girders, cross beams, brick arches and covering, average 24 lbs. per square foot. The possible weight of crowds of people upon the floors is taken at 100 lbs. per square foot; the snow upon the roofs, at 2 feet depth, is 12½ lbs.; and the effect of the strongest winds, which may at times be deflected perpendicularly against some of the roofs, is taken at 15 lbs. per square foot. The calculated weight which may come upon each of the walls is as follows: on the corner towers and front foundation walls, 47 tons per lineal foot; on the main east and west front, 50 tons; on the curtains, 44½ tons; on the ventilating tower, 67 tons; on the division walls, extending upwards through four stories, 23½ tons; on the partition walls of two stories, 13½ tons, and of those which extend one story high, 8½ tons, per lineal foot. The main tower is designed to be of stone, except the portion immediately below the dome, which, from being so high from view, was proposed to be made of iron. If it should be of stone to the dome, that change, together with some others, would increase its weight to 36,000 tons, equal to 14.4 tons per square foot at the base. Its footing stones were spread to 110 feet square, and the concrete to 125 feet square, and 5 feet thickness. The weight on the clay, with 30,000 tons, is 1.92 ton per square foot; and with 36,000 tons, it would be 9.3 tons; but it was arranged for an underpinning, if necessary.

THE EXPERIMENTS.

For the purpose of ascertaining the

sustaining power of the blue clay in its natural condition, two sets of experiments were made; in the first by pressure upon a square foot, and in the second upon a square yard, of the surface. The machine used was a mast of timber 12 inches square, held perpendicularly by guys, with a cross frame for the weights. A hole was dug, 3 feet deep, in the bottom of the blue clay foundation, 18 inches square at the top, and 14 inches at the bottom. The foot of the machine was placed in this hole, and weights from 2,754 lbs. to 23,784 lbs. were applied. Small stakes were driven into the ground, in radial lines from the center of the hole, and the tops carefully driven to the same level; and by means of a straight-edge any change in the surface of the ground adjacent to the hole could readily be detected and measured.

Table I. shows a continued settlement of the clay under the foot of the machine as the loads were added, but no change in the surface of the adjacent ground was observed until an hour after a weight of 11,844 lbs. had been applied, when an uplift of the surrounding earth was noted, in the form of a ring with an irregular rounded surface, the contents of which, above the previous surface, measured 0.09 cubic foot, which is equivalent to a displacement of 1.09 inch of clay in depth under the foot of the machine, or equal to one-fifth of the whole settlement which had then occurred.

TABLE I.

Observation.	Day.	Hour.	Duration.	Weights.		Settlement.		
				Each.	Total.	Each.	Total.	
			Hours	lbs.	lbs.	Inches.	Inches.	
1	Mon.	5 P. M.	—	2,754	2,754	0.288	0.288	The weight of the machine.*
2	Tues.	9 A. M.	16	—	—	0.612	0.900	
—	"	9 "	—	2,820	5,574	—	—	First stone added.
3	"	10 "	1	—	—	0.528	1.428	
—	"	10 "	—	6,260	11,834	—	—	Second and third stones added.
4	"	11¾ "	1¾	—	—	4.752	6.180	Uplift noted.
—	"	11¾ "	—	3,250	15,084	—	—	Fourth stone added.
5	"	¾ P.M.	¾	—	—	3.588	9.768	
6	"	1 "	½	—	—	1.728	11.496	
—	"	1 "	5 min.	2,890	17,974	0.060	11.556	Fifth stone. Immediate settlement.
7	"	1½ "	½	—	—	3.288	14.844	
—	"	1½ "	—	2,980	20,954	—	—	Sixth stone added.
8	"	2 "	½	—	—	4.128	18.972	Uplift noted.
—	"	2 "	—	2,830	23,784	—	—	Seventh stone added.
9	"	2½ "	½	—	—	5.184	24.156	
10	"	3 "	½	—	—	2.060	26.216	
11	"	4⅓ "	1⅓	—	—	1.300	27.516	
12	"	5 "	1	—	—	3.084	30.600	Uplift noted.
13	Wed.	5 A. M.	12	—	—	0.600	31.200	
14	"	8 "	3	—	—	—	31.200	No settlement after 5 A. M.

Observations 12 and 13 are not reliable.

* The first settlement, noted in observation 1, was due to the weight of the machine, and was not a compression, but only a leveling of the rough inequalities of the clay. The subsequent observation 2, of 0.612 inch, is the compression due to 2,754 lbs. This settlement occurred before 5 A. M.

When the weight had reached 20,954 lbs., and had rested for half an hour upon the clay, a further protrusion was noted. The form of the ring was the same as before, but with more irregularity of surface. The highest part of the protrusion was from 12 to 15 inches from the edge of the pit, where it averaged 0.3 inch high, and sloped off outwardly to an average of 4 feet from the center of the hole. This uplifted earth measured 0.606 cubic foot, which is equivalent to a displacement of 7.272 inches. When a weight of 23,784 lbs. had been applied, and had rested three hours on the clay the ring in the highest part averaged 0.5 inch high, in the same general form and extent as before noted. The amount of earth thus raised was 1.01 cubic foot, equivalent to a displacement of 12.12 inches under the machine.

Before the lifting of the earth surrounding the machine could have taken

place, the materials first displaced from under the machine were doubtless forced among the particles of the earth adjacent to the whole, and compressed that earth to some extent; and this operation was continued until the adjacent earth had become so compacted as to cause the lifts noted in the table. The author is of opinion that the compression of the earth below the bottom of the machine continued without any considerable displacement until after a load of 4,000 lbs. or 5,000 lbs. had been applied, and that then the displaced earth found space in the adjoining earth until the load reached 7,000 or 8,000 lbs., when the uplift became visible at the surface of the ground; but that meanwhile the earth directly under the machine was continually more and more compressed in some proportion to the weight added. The small area pressed upon facilitated the escape of the material into the adjacent earth, which weighed only 300 or 400 lbs. per square foot. If the pit had been deeper, or the piston larger, there would have been less displacement.

The second set of experiments was made with the same machine, to the bottom of which was framed a strong base, 3 feet square. The pit was sunk 2 feet deep into similar earth, and was 38 inches square both at the top and at the bottom. The stones were put on at intervals of an hour. There was no uplifting of the surrounding earth.

Table II. shows the remarkable regularity in the settlement as the load was increased, and a constant diminution of the increment as the earth became more compacted. At the 6th observation the weight per square foot corresponded nearly with the 2d in Table I., and the settlement was almost the same. The base was nine times as large, so that the proportion of escapement of the earth from beneath must at this time have been very small. It is probable, however, that if the weights per square foot had been increased so as to equal those in Table I., a similar uplift would have occurred, though of less extent. The author derived from the Tables the opinion that the extreme supporting power of this earth was less than 6 tons per square foot, and that the load which might be safely imposed upon the clay was 2 tons per square foot.

TABLE II.

Observation.	Weight.			Settlement.		
	Each.	Total.	Per sq foot.	Each.	Total.	
	lbs.	lbs.	lbs.	Inch.	Inch.	
Machine Placed.....	3,228	3,228	359	—	—	No settlement that could be measured.
1st stone added.....	2,380	5,608	623	0.050	0.050	
2d " "	3,300	8,908	990	0.150	0.200	Estimated.
3d & 4th stone added.	6,960	15,868	1,763	0.166	0.366	
5th " "	2,830	18,698	2,078	0.134	0.500	"
6th " "	3,250	21,948	2,439	0.124	0.624	
7th " "	4,420	26,368	2,929	0.096	0.720	"
8th " "	1,190	27,558	3,062	0.080	0.800	
9th " "	2,320	29,878	3,320	0.025	0.825	

The notations of the settlement were generally made about an hour after the weight had been applied.

For the purpose of maintaining the clay beneath the structure in the same condition of moisture, a deep puddle wall was extended entirely around the foundation, not only to exclude an excess of water, which might reach it through the veins and films of sand with considerable hydrostatic head, but also to prevent the egress of the natural moisture through similar veins. Although the puddle wall was carried up to the level of the terrace which surroundsthe building, yet water might find its way along the face and down the outside of the walls, or possibly through some accidental break in the concrete floors within and surcharge the clay below. To prevent this, there was spread on the top of the clay, over the whole area enclosed, a depth of 6 inches of coarse screened

gravel, the effect of which will be that under the great weight of the building any excess of water in the clay beneath will be forced into this pervious gravel, and flow off through it to the drains which encircle and traverse the foundations. The necessity for these provisions will be apparent when it is considered that many of the veins of sand extend to the surface of grounds of much greater elevation than the foundations, and that they communicate with imperfectly built street sewers and water pipes, while the same or other porous veins extend beneath the surface to grounds which are much lower. Through these sources the clay under some portions of the structure might be charged with water, while that under an adjacent wall might, at the same time, be drained of much of its natural moisture, and thus entirely destroy the design of a foundation which should everywhere have an equal sustaining power. It is not an absolute settlement which is to be apprehended, but a greater yielding in one place than in another.

A common practice of builders who have occasion to erect high and comparatively heavy towers and spires, is to groove the lower part into the adjacent walls, so as to allow the heavier ones to slide in these grooves, without breaking the bonding stones. In the present case the demands of the architect forbad the use of grooving, and hence the necessity for the above provisions.

The main walls of the building are from 5 feet to 7 feet thick, where they rest upon the foundation walls, and bring upon them pressures of from 6 to 9 tons per square foot, which had to be reduced to 2 tons per square foot on the clay. This was accomplished by projecting each of the footing courses beyond those immediately above them. The rule was to commence with a load of 2 tons per square foot upon the clay, 3 tons upon the top of the concrete, and generally 4, 5, 6, or 7 tons upon each succeeding course of stone. The weight on each lineal foot of the top of the foundation walls, divided by the above pressures, gives the exact width of each course of the footing stones, as shown in Table III.

TABLE III.

Parts of the Building.	Load per Lineal Foot.	Required width of Courses.										Main Walls.
		Concrete.		Courses of footings.								
				1st.		2d.		3d.		4th.		5th.		
	Tons.	Ft.	Ins.	Ft.	Ins.	Ft.	Ins.	Ft.	Ins.	Ft.	Ins.	Ft.	Ins.	Ft. Ins.
Corner towers, front	47	33	6	15	8	11	9	9	4	7	10	7	4	7 0
" " rear..	39	19	6	13	0	9	9	7	10	6	6	5	6	5 0
Curtains, front.....	44½	22	3	14	8	11	1	8	11	7	5	6	5	6 5
Central fronts.....	50	25	0	16	8	12	6	10	0	8	4	7	2	6 5
Partitions, 4 stories..	23½	11	9	7	10	5	11	4	9	3	11	3	5	3 0
" 2 "	13½	6	9	4	6	3	5	3	0	—		—		3 0
" 1 "	8½	4	3	2	5	2	2	2	0	—		—		2 0

The large quantity of stone required in a short time—50,000 tons in four months—compelled a resort to a great many quarries, which furnished stones of different thicknesses, and made it necessary to modify the above exact arrangement; but the principle of the distribution of the load according to the vertical strength of the stone used was maintained throughout the foundations.

It was necessary to consider how far these projections could be made without danger of breakage of the projecting part of the stone. The pressure in this case tending to break the stone is that due to the weight on the wall above it, divided by the width of the wall and multiplied by the area of the projection, and to treat that result as a load distributed on a beam supported at one end.

To distribute the weight upon the footing stone courses with certainty, the beds of the limestone and granite were

dressed to close parallel joints, so that the weight of each of the upper courses should be carried out to the extremity of the next course below. The vertical joints were only required to be quarry joints, not exceeding 1 inch wide. For certainty and convenience of laying the masonry, the foundation stones were all required to be rectangular blocks, of from 18 to 24 inches in thickness, the breadth to be at least one and a-half time the thickness, and the length two and a-half times the thickness. The average size of all the stones was 31 cubic feet, equal to 2½ tons, and many of them were from 5 to 8 tons weight. In the foundations of the main tower the average weight of the granite blocks was four tons, and of the projecting blocks 7 tons. The footing courses were spread out equidistant from the lines of the center of gravity of the imposed weight above. The exterior stones of the three lower footing courses were all headers from 4½ to 7 feet in length. The longitudinal bonding was made by the interior stone, and in the upper courses, where the projections were smaller, by alternate headers and stretchers of the front stone, as well as the interior. The result of this bonding will be to distribute the weight, and equalize its pressure upon the clay.

The weight of the main tower was so much greater than that of the other walls, and the earth below it so much inferior, that the foundation was placed 7 feet deeper than elsewhere. With this exception, all the walls were commenced at the same level. The spaces between the main exterior, rear and division walls, and under the arches of the central court, were covered with a layer of concrete, made of screened gravel and hydraulic cement, 1 foot to 2 feet thick.

THE COMBUSTION OF FUEL IN FURNACES OF STEAM BOILERS BY NATURAL DRAUGHT AND BY SUPPLY OF AIR UNDER PRESSURE.

By Mr. JAMES HOWDEN.

From "Iron."

The various improvements in the steam-engine which have led, during the last twenty or thirty years to the great reduction in the ratio of fuel consumed to power obtained, have hitherto been almost exclusively confined to the motor, and scarcely, if at all, connected with the generator; the reduced consumption having been effected by the more judicious use of steam in the engine, and not by its more economical evaporation in the boiler. Putting aside the merely constructional improvements arising from greater experience, better material and appliances, steam boilers for land and marine purposes are practically the same as they were when the average consumption of coal per horse-power developed in the engine was about three times that of the present day. It may even be said that we have retrograded rather than advanced, as forty years ago a higher evaporative economy was prevalent in Cornwall than is found there or anywhere else now. In that district, in 1846, an evaporation was recorded per lb. of coal of 12.89 lbs. of water from 212°. In the marine practice of the present day the average evaporation per lb. of coal is probably not more than 0.6 of this Cornish economy, while in those high-speed steamships which maintain an average of about 17 knots across the Atlantic, it is probable that barely half of this evaporative duty is reached. This great falling off in evaporative economy in boilers of the present day must not, however, be ascribed to less knowledge, but to the different conditions and necessities imposed by great industrial progress. The conditions under which the high economy of the Cornish boiler was obtained are incompatible with marine requirements. They would also, as a rule, be impracticable or unprofitable if applied to land boilers, especially in populous districts. Time and working space are every day growing more valuable. A maximum of power in a minimum of space is being more and more sought for.

This demand is diametrically opposed to the essential conditions by which the high Cornish economy was obtained. The rate of combustion was from 3 to 4 lbs. of coal per hour per square foot of grate, accomplished by careful restriction of the air admission to the furnaces. The proportion of heating surface to water evaporated was from six to seven times that of the average in marine boilers. A comparatively low temperature in the high chimney gave an adequate draught for the slow combustion, and permitted the heating of the feed water by the waste gases after leaving the boiler. Add to these precautions the most thorough protection of the whole boiler from loss of heat by radiation, and we have the conditions by wbich the high evaporative economy was effected in Cornwall forty years ago.

Though the necessities of our day render this cumbrous and slow system unsuitable for our steam supply, yet it must be acknowledged that the fact of the higher evaporative power now required from boilers being obtained only with a great sacrifice of evaporative economy is inconsistent with the general advance in engineering science, and contrasts unfavorably with the marked economical improvements in the engine or motor. I hope, however, to show that this anomaly is not incapable of being removed, and that even a much higher evaporative power than now prevails may be had from boilers, along with a higher evaporative economy than is found in present practice, that, in short, an approach may be made to the unequaled economy of the Cornish slow combustion system with an evaporative power, it may be ten times greater. Experience has hitherto shown generally that, with slow combustion judiciously carried out, a high evaporative economy has been effected by natural draught, but as the rate of combustion has increased the evaporative economy has steadily fallen. Further, when combustion has been raised by forced blast to a rate much higher than is possible by natural draught, the evaporative economy has decreased in a still higher ratio. As it is important at this stage to elucidate the causes of this unsatisfactory result, I will endeavor to do so before describing how the difficulty may be overcome.

Combustion by natural draught in boilers at the rates now prevalent is necessarily more or less wasteful and imperfect, because the velocity given to the air supply, even with the sharpest draught, is insufficient to cause it to penetrate and mix with the fuel over the whole area of a furnace of ordinary dimensions, and further, because a very considerable portion of the heat of combustion is required to rarefy the air in the chimney to maintain the draught, this expenditure increasing with the rate of combustion. Unless, therefore, a much greater supply of air is admitted to the furnace than is required theoretically for perfect combustion, it is found that, owing to unequal distribution and imperfect mixture of the air with the fuel over the area of the furnace, part only of the oxygen enters into union with the combustible gases and carbon of the fuel, the remainder passing to the chimney unassimilated. The unavoidable consequence is that a considerable portion of the carbon leaves the boiler unconsumed as carbonic oxide. Smoke is also formed in large quantities, and though this objectionable product contains in itself but a small portion of carbon, it is always an indication of imperfect combustion, and of more important constituents of the fuel being wasted. On the other hand, if, in order to secure the more complete combustion of the fuel, an increased admission of air is made to the furnace, then the excess of air—which must always be largely beyond the quantity required for perfect combustion when properly carried out—reduces the temperature of the furnace, and consequently the ratio of evaporative power, and, what is still more wasteful, robs and carries off to the chimney a large percentage of the total heat of the fuel.

The wastefulness arising from imperfect combustion caused by restricting the air supply as described, may be estimated from the following data. When perfect combustion is effected by the chemical union of two atoms of oxygen with one of carbon forming carbonic acid, 14,500 units of heat are evolved from 1 lb. of carbon, but when from imperfect combustion only one atom of oxygen unites with one atom of carbon and carbonic oxide is formed, only 4,400 units of available heat are evolved from one lb. of carbon. The

loss arising from this imperfect combustion can therefore be calculated by ascertaining the proportion of carbonic acid to carbonic oxide in the waste gases, noting also the quantity of air passing away with its oxygen unassimilated. The wastefulness arising from admitting excess of air in order to obtain more complete combustion and reduction of smoke, is estimated by ascertaining the quantity of air admitted in excess. About 11 tons of air are required for the perfect combustion of one ton of ordinary coal, if no portion of the air is wasted. Double this quantity, or even a larger proportion is frequently supplied. If double the quantity be assumed, this 11 tons of superfluous air must be heated from the combustion of the one ton of coal, and not only greatly reduces the temperature of the furnace, and its evaporative power, but in leaving the boiler at a temperature, it may be 600° or more, higher than it entered the furnace, it thus carries off a large percentage of the heat of the fuel. The loss stated in figures caused by this 11 tons of superfluous air leaving the boiler at 600° above the entering temperature is $11 \times 2240 \times 0.246$ (the specific heat of the escaping gases) $= 6061.4$, which $\times\ 600 = 3{,}638{,}840$, the loss in units of heat, being fully $12\frac{1}{2}$ per cent. of the total heat of combustion of the ton of coal, taking the efficiency of the coal at 12,600 units per lb.

The dilemma, therefore, in which every furnace operator finds himself when working with natural draught, is the choice of admitting either a moderate amount of air to the furnace, and having in consequence an imperfect combustion, with much smoke and considerable waste of fuel, in carbonic oxide passing to the chimney, or of admitting air greatly in excess of what is required for perfect combustion properly effected, and obtaining therefrom a more complete combustion at the cost of a large waste of heat carried off to the chimney by the superfluous air, besides the reduction of the evaporative power of the boiler by the dilution of the furnace temperature. Of these two sources of loss, unfortunately, the greater is generally that arising from the too liberal supply of air, consequently the hotter furnace, with increased steam and much smoke, is preferred by all stokers to the cooler furnace with diminished steam and less smoke. We thus find, unless it is restricted by the most severe penalties, smoke polluting the atmosphere in every place where steam power is used, hanging over our manufacturing towns like a cloud, and marring the most beautiful rural scenery, as well as our finest rivers and estuaries; a nuisance which apparently owes its toleration to its universality.

The imperfections such as enumerated attending the combustion of coal by natural draught, with the limitation of the supply of steam, often further effected by the state of the draught and quality of coal, have led to frequent attempts during the last thirty or forty years to effect combustion in boiler furnaces by air supplied by mechanical means above atmospheric pressure. These repeated attempts, however, have not hitherto been successful, and, therefore, except in locomotives and torpedo boats, supply of air by natural draught is still in universal use for steam boilers. The causes of failure, though various and often not at first sight apparent, become, nevertheless, obvious when sufficiently investigated. A short description of the most prominent of these attempts to improve combustion by mechanical means may be of some interest. The plan which has probably been most frequently tried is that of forcing air by a fan or other blower into a closed ash pit. This mode of increasing the power of a furnace, though apparently simple, must always be disappointing, for the following reasons:—If the coal on the fire-grate be kept sufficiently thin to permit the oxygen of the air supply to penetrate to the upper layers of the fuel to effect combustion of the inflammable gases, the quantity of air passing through will be too great, and the temperature of the furnace will be unnecessarily reduced, and much loss of fuel occur. If, on the other hand, the coal be laid sufficiently thick to prevent an undue quantity of air from passing through, there will be a lack of air to consume the combustible gases generated by the great heat from the combustion of the lower layers of fuel. Much smoke will in consequence be produced, and also carbonic oxide, so that in this case also less economy will follow than may be realized by natural draught. This system of supplying air to a furnace is likewise very severe on the fire bars, and has other disadvantages which time will not permit reference

to. It is now exactly twenty-two years since I constructed a boiler to try this mode of effecting combustion by air under pressure. The boiler was erected in the yard in which it was built. It had a short chimney, and the air was supplied to the closed ash pit by an engine and fan. The results of the trials were disappointing, and exactly of the character I have described. They were not, however, altogether valueless, as they clearly showed that forced combustion could not satisfactorily be carried out on this system.

A second method resorted to for increasing combustion in boiler furnaces is that of exhausting the air in the chimney or uptake by a fan, thus reducing the pressure in the flues or tubes and furnaces, and thereby inducing a more rapid current of air through the furnace both below and above the grate-bars. This plan, so far as the supply of air to the furnace is concerned, is in practice more workable than the plan previously described, as the air enters the furnace not only at a greater velocity than is attainable by natural draught, and is thereby capable of being more thoroughly intermingled with the fuel all over the furnace. but being also balanced in pressure above and below the grate-bars, the operations of the furnace become much more easily managed. This mode of creating these advantageous conditions in the furnace is, however, objectionable. The passing of the hot gases of combustion through a working fan of itself a mistake, practically and theoretically. Even if it were possible for the machine to continue in working order under this ordeal for any length of time, the hot gases, if leaving the boiler at no more than 480° above the entering temperature, would be twice the volume of the air which entered the furnace from the stokehold. A fan to exhaust the hot air would, therefore, require to be at least double the capacity of one which would have supplied the same quantity direct from the stokehold to the furnace. This plan, which is one of the earliest tried, is therefore impracticable for large boilers, and has only been occasionally used in boilers of a limited size.

A third method producing the same effect on the furnace as the last is that of inducing a current by means of a steam jet in the funnel. This plan is simple, requires no working machinery, and is within certain limits effective. It illustrates the advantage of mixing the air more thoroughly with the fuel than is possible by natural draught, as by its use a greater evaporative power is obtained, while the consumption of fuel per horse-power of the engine is not more than the rate per horse-power for natural draught, notwithstanding the loss of steam by the jet. The waste of water by this system unfits it for marine boilers working at high pressure, and it is not required in land boilers with high chimneys. It is, however, seen on a grand scale, and in its most legitimate use, in the locomotive boiler, where, instead of merely a small percentage of the steam from the boiler being used for the purpose of inducing a current through the furnace, the whole of the steam generated is used for this purpose. The remarkable effect of the powerful action of the blast pipe is well known. It has increased the power of the boiler at least six-fold that possible by natural draught. What we owe to this power of increased combustion is simply incalculable. It is not too much to say that the amount of traveling and carriage of goods from end to end of the kingdom could not have reached a tithe of its present enormous extent but for this powerful artificial supply of air to the furnace of the locomotive boiler. An average consumption of 100 lbs. of coal per square foot per hour is burnt on the fire-grate of an express locomotive, and what should not be overlooked in the consideration of this subject of combustion under air pressure, this astonishing rate of combustion is accomplished with comparative ease and remarkable economy.

A further method of improving combustion, and especially of effecting smoke prevention, which has been frequently tried, is that of increasing the supply of air to the fuel above the grate by currents induced by steam jets. The steam jets penetrate the fuel, carrying with them the air to supply the necessary oxygen to parts of the furnace which would not be sufficiently reached by natural draught. Combustion by this means is improved, and smoke in large measure prevented. This is not in itself an economical plan, and it has several objectionable features. For marine boilers it is for various reasons unsuitable. It gives another

proof, however, of the advantages, within limits, of an artificial supply of air above atmospheric pressure, as the better combustion it promotes compensates to a considerable extent for its inherent wastefulness.

The most successful plan, in some respects, that has hitherto been applied to marine boilers is that of the air-pressure stokehold or boiler-room, as used in the torpedo-boats—a system so well known to the members of this institution as to render description unnecessary. The air enters the furnace above and below the fire-bars in the same manner as by natural draught, but at the increased velocity due to the difference of air-pressure in the boiler-room over that of the atmosphere outside. The results obtained in evaporative power approach that of the locomotive boiler. The comparatively small boilers used in these boats, generally of the locomotive type, under the powerful action of the great supply of air to the furnace under pressure, generate steam sufficient to impart a speed of twenty-five or twenty-six miles an hour to the vessel. The success of this system in torpedo-boats has led our own and other governments to apply it to large steamers having a number of boilers with several furnaces in each. It being impracticable to make air-tight a large boiler compartment, and the coal bunkers which must necessarily be in open communication therewith, the plan has been adopted of forming air-tight chambers, or rooms, on the furnace ends of each boiler, or pair of boilers, into which the air is forced by fans working on the deck above. These rooms contain the coals being used by the stokers on duty. Access to the chambers is obtained by doors designed to prevent egress of air. At the meetings of the institution last year a most interesting paper was read by Mr. R. J. Butler, giving much valuable information regarding the application of this system to H. M. steamships *Satellite* and *Conqueror*. The results of the trials in these steamers show that much larger supplies of steam can be obtained by this system with less discomfort to the stokers than would have been possible from natural draught.

This statement appears, however, to exhaust all that can be said in favor of this system. Its practical disadvantages are very considerable, even in war vessels, and are more than sufficient to prevent its application to mercantile steamers. The chief defects of the system—and they are serious defects—are its wastefulness and its tendency to injure the boilers. It is a system which may be characterized as one for obtaining an increase of power regardless of expense. After experiencing the impossibility of working a furnace with air under pressure without waste of fuel, unless most carefully studied precautions are adopted, I can state without hesitation that the indiscriminate admission of air in this system must be wasteful of fuel in a very high degree. The effect on the boilers themselves, whenever a furnace door is opened, of the rush at full pressure of a column of cold air, equal to the area of the furnace door, at a velocity from 80 feet to 100 feet per second, must be most injurious. The furnace plates, as well as the tube and other plates of the combustion chamber, cannot but suffer severely, while the great volume of cold air lowers the temperature of the whole boiler, and greatly neutralizes the benefit of the rapid combustion of fuel. Mr. Butler did not state in his paper the consumption of coal per horse-power developed by the engines when at their highest power, but indicated that when the power was much increased beyond that obtainable by natural draught, the expenditure rose largely out of proportion to the increase of power.

Mr. Butler also stated that in the *Satellite*, after a few trials, each of several hours' duration only, "a number of the boiler tubes were leaky," and in the *Conqueror*, after similar trials, that "both seams and tube-plates were leaking," and expresses the opinion in regard to the endurance of the boiler, "that the frequent use of the forced draught would produce a great diminution in the life of those parts subjected to the intense heat." From experience in working boilers on a large scale in a similar manner, that is, with sudden alterations of heating and cooling, I can fully endorse Mr. Butler's opinion regarding the short life of the boilers, attributing, however, the injury to the boilers not to the intense heat, but to the sudden changes of temperature. From the fact that the indications of injury in the boilers of the *Satellite* and *Conqueror* were not greater, I would in-

fer that the water used was fresh, or, at least, that the plates were entirely free from scale when the trials were made. When scale is formed on the plates to any extent, the injury from this mode of working becomes very marked. Altogether, it appears to me that this system of increasing combustion is, from its wastefulness and the defects I have mentioned, unsuitable for continuous working in any large steamer. For torpedo boats with one furnace, where the disadvantages are minimized, this system may hold its place, and as these boats are only occasionally used, waste of fuel is, therefore, in them of small importance.

I now come to describe a system of increasing combustion artificially, which I devised during the year 1880, having been led to this arrangement by the remembrance of the causes of failure to obtain satisfactory results in 1862, already alluded to, and also by observing the defects of the different methods to which I have referred. In the summer of 1882 I had an opportunity of applying it to a small marine boiler, 7 feet diameter by 7 feet 9 inches in length, having two furnaces each of two feet diameter. The results being satisfactory, I constructed a boiler for the purpose of testing on a sufficiently large scale the various important advantages contemplated by the adoption of this mode of working. This boiler is 10 feet in diameter by 9 feet in length, with two furnaces, each 3 feet inside diameter. The furnace tubes extend the whole length of the boiler, the fire gases returning to the front from a dry chamber at the back end lined with fire-brick and otherwise protected from radiation of heat, through two separate stacks of tubes, each stack having 45 tubes, 3¼ inches external diameter. This boiler I erected in a yard by itself in July last, with evaporating tank, engine and fan, and other appliances suitable for testing results, and have since then been engaged more or less frequently in making trials under various conditions. The principal ends which I have endeavored to accomplish satisfactorily are:—

(1.) To effect with ease and certainty complete combustion of fuel in the furnaces of ordinary steam boilers at all rates from zero up to that of a locomotive boiler if required.

(2.) To effect this combustion at all rates, whether high or low, economically as regards fuel.

(3.) To effect this economical combustion rapidly or slowly in boilers of ordinary character, either singly or in numbers, without any change in the usual conditions of stokeholds for boilers worked by natural draught.

(4.) To effect the foregoing purposes with less discomfort to the stokers and attendants, and with less wear and tear of the boilers and furnace fittings, than is experienced when working in the ordinary way with natural daught.

To attain the ends sought for under the first head made it necessary to use air under a pressure above the atmosphere, such as is conveniently got from a fan, with simple means of using more or less of this air as required. To attain the second point, that of economical combustion at all rates, it was imperative that the supply of air should not only be under perfect control, but that the necessarily varied quantities admitted per unit of time or of coals consumed, should be exactly ascertained; and, further, its distribution should be as nearly as possible equal over the furnace, and limited as nearly as possible to the theoretical quantity for the weight of fuel being consumed; finally, that the waste of the heat of combustion should be reduced to a minimum. To accomplish the third, it was necessary that the supply of air to any single furnace in a boiler, or to any boiler in a series, should be quite independent of the supply to the other furnaces or boilers. The advantages under the fourth head follow from the more economical combustion requiring less supply of fuel than would be necessary with natural draught, by the radiation of heat into the stokehold from the furnaces being prevented, and by the supply of air to the fan being made to circulate through the stokehold. The saving of tear and wear is due to the air being introduced to the furnace so as to preserve the furnace fronts and interior fittings, and also to the prevention of the usual injurious rush of cold air into the furnace when stoking, by suspending the air admission when a furnace door is open.

The manner in which I have endeavored to carry out these several purposes will be understood more clearly by the following description: On the front end of the boiler an air-tight chamber or res-

ervoir is formed of light plate iron, to receive the air for combustion under pressure from a fan. This air chamber extends over the whole front of the boiler, from some distance above the upper row of tubes downwards, so as to enclose the furnaces and ash pits. The smoke-boxes of each stack of tubes are completely separated from the air chamber by suitable castings, as are also the furnaces and ash-pits, so that no air can enter the furnaces or ash-pits except through passages regulated from the outside by valves. The upper portion of the reservoir extends outwards from the boiler not more than the ordinary smoke-boxes and uptake; below, at the furnaces, the reservoir in this case extends 7½ inches from the boiler, but this may be reduced. On the front of the castings which separate the furnaces from the air chamber, the outer furnace doors and ash-pit doors are hinged air-tight. Attached to the outer furnace doors by studs, and moving on the same hinges, are the inner and proper doors of the furnaces, which shut on the inner front plate in the usual manner. A dead plate between the outer and inner front plates separates the ash-pits from the furnaces above the fire bars. Into these spaces above the dead plate and between the outer and inner furnace doors the air is admitted in the desired quantity from the common chamber by simple plate valves, and after passing through perforations in the doors, at the sides, and above the doors, is received into cast iron boxes, which serve the double purpose of protecting the front plate and furnace door from the great heat, and distributing the air effectively among the fuel. The air is admitted as required to the ash-pits by separate valves.

By these arrangements the admission of the air to different parts of the furnace is kept under perfect control, the quantities admitted being ascertainable, and all radiation of heat from the furnaces or ash-pits prevented. The heat of the fire gases after they leave the boiler is largely utilized by causing them to pass up through the tubes in the air reservoir immediately above the smoke boxes, as shown. While the hot gases pass through the inside of these tubes, the cold air from the fan entering by the pipe is made to sweep right and left among the tubes, carrying a considerable portion of the escaping heat downwards by the sides of the smoke boxes to the furnaces. When a furnace or ash-pit door is opened for stoking purposes, the air admission valves for that furnace are closed, so that the furnace may be operated on in a quiescent state. No rush of cold air can thus pass into the furnace, as in the air pressure stokehold system, or even as in boilers worked by natural draught. The saving of the boiler from injury, and the prevention of waste of fuel, by this means are important features. When it is desired to reduce or increase the rate of combustion, the admission valves are opened or shut as required, and with any given pressure of air in the chamber, it is not difficult, after a few trials, to establish the required openings for different rates of combustion, from zero to the highest rate possible by the given air pressure. When the admission valves are entirely shut the combustion may be practically suspended for hours, the fires continuing more or less incandescent. By this means a steamer may with ease be kept lying under any desired pressure of steam for any length of time, and without tendency to blow off.

In erecting the boiler for trial, I purposely made the conditions under which it was to work as difficult in regard to combustion as would ever be likely to occur under the most unusual circumstances in actual practice. The funnel was made only 15 inches in diameter and 21 feet high from the fire grate. A still greater restriction was the limiting of the air heating tubes in the reservoir (through which the whole of the fire gases issuing from each furnace have to pass to the common uptake) to fourteen in number of 3½ inches external diameter. The effect of these restrictions and contractions is to reduce largely the rate of combustion with a given pressure of air. With natural draught this boiler, as described, generates steam very slowly. With 2 inches to 2¼ inches of air pressure a combustion of about 30 lbs. per hour per square foot of fire-grate can be reached. This rate could evidently be doubled with a corresponding increase in outlet area, or by increasing the air pressure, but the latter would be undesirable where the former can be accomplished. Under the restricted conditions described, the boiler

is worked with the utmost ease at any rate of combustion up to the limit mentioned, and with absolute freedom from smoke. In the numerous trials which have been made during the last seven months I have endeavored to ascertain the following particulars :—

(1.) The rates of combustion effected by admissions of air varying both in quantity and pressure.

(2.) The effect of different quantities and velocities of air admission on evaporative economy.

(3.) The most favorable modes and proportions of admission and distribution of air under different pressures.

(4.) The nearest approach to perfect combustion chemically at various pressures with the least possible admission of air.

It will be evident that the elucidation of these various particulars with tolerable accuracy requires a great number of trials, and as each trial is frequently of five or six hours' duration to ensure accuracy, a great expenditure of time is involved. Though the trials have been going on for the time mentioned at about an average of one per week, they are not yet so complete as to enable me to establish definitely the best proportions and velocities of air admissions above and below the fire-grate for all rates of combustion. I can as yet only give approximations, though the conditions which yield the most favorable results are more or less ascertained. As I mean to continue these trials, I hope to be able to give fairly complete statements on some future occasion. At present the time at my disposal will only allow of a few remarks describing the mode in which the trials have been carried out. In ascertaining the effect of the admission of air above as well as under the fire-grate, trial was made with different areas of apertures, and of different pressures of air on equal areas. For example the trials were begun with a limited aggregate area in the holes of the air-boxes inside the furnace, this area being gradually increased until it was found that the increased area added little to the rate of combustion, while the evaporative economy became sensibly reduced. The admission below the grate was also tried with various areas of air inlet apertures and pressures, and their effect noted on the combustion and evaporation.

In the course of these trials some interesting results were noticed. For example, when the air admissions above and below the fire-bars were regulated in certain proportions to each other, if the admission openings above were then decreased, and those below increased to about the same amount, a much higher rate of combustion took place than when the process was reversed by increased admission above and decreased below. This difference arose from the changes produced in the balance of pressure in the furnace. When the pressure is relieved somewhat above and increased below, the air passes much more rapidly through the fuel in its passage to the chimney, with a correspondingly more rapid combustion, than it does if, with the same aggregate admission, the pressure is increased above and decreased below. In this case the current upwards through the fuel is greatly checked, and proportions could be so regulated that the current through the fuel could be nearly prevented by the pressure above. At the same time the admission of air above the fuel in proper proportions serves most important purposes, and contributes towards realizing the highest economy. It has also an important effect in preserving the furnace fittings and fire-bars. I have referred to the great destruction of fire-bars caused by working with the closed ash-pit system. The cause of this destruction became evident to me after I had several trials with the new system I am describing, as in it the admission above the bars tempers the velocity of the air through the fuel from below, while the fuel is kept in perfect combustion above. When the pressure of air is entirely from below, as in the closed ash-pit system, with a sufficient depth of fuel on the grate and a given air pressure, the bars can be melted by the intense heat generated in the lower layers of the fuel. I find, on examination, the fire-bars and air boxes in the boiler I am operating with are still sharp on the corners, and as good now as when put in, after making with them all the trials I have mentioned, and consuming in some cases about 30 lbs. of coal per square foot per hour. This is an important practical result in the working of the furnaces with air under pressure.

The chief point, economically, towards which I have been working in these trials is the attainment of a high rate of combustion with a minimum admission of air,

that is, an admission as near to the theoretical limit as can be practically reached. This is no easy matter, and requires to be worked towards step by step. The trials would have been in great measure a groping in the dark, even with the appliances described, but for the apparatus devised to ascertain by measure of the quantity of air entering at different parts of the furnace above and below the firebars at the various pressures used. This device, which occurred to me after several trials were made, is the application of an anemometer, with an apparatus in which the same conditions as to entering and back pressures are created as those which exist in the part of the furnace into which the air is being admitted. The velocity is read off without difficulty, and the dimensions of apertures being known and temperature ascertained, the volume of air is exactly calculable. The system of measurement, combined with chemical analysis of products of combustion, enables one to work with intelligence and certainty. These trials have strikingly shown the great difficulty of obtaining a high rate of combustion with forced blast, without admitting a most wasteful excess of air. It is comparatively easy to obtain a high rate of combustion, but very difficult, unless suitable adaptations are used, to combine a high rate of combustion with economy. In the small boiler, which I first tried with a very complete combustion, the evaporation was very low per lb. of coal, entirely owing to the excess of air. In the present boiler the evaporation has already reached a very fair economy, though not within a considerable amount of what is yet almost certain to be attained.

What has already been accomplished shows that from 9½ to 10 lbs. of water, at 212°, could be evaporated in boilers at sea, from 1 lb of Scotch coal, with a rate of combustion of 30 lbs. per square foot of grate per hour; but there are good grounds for expecting that an evaporation of even 12 lbs. may yet be reached with a rate of combustion from 40 to 50 lbs. per hour per square foot of grate. It should be explained that the boiler I have used for these trials, with the furnace arrangements described, is not intended to represent the best mode of carrying out this system of combustion. It is merely a boiler suitable for ascertaining results under various conditions. There are several changes now being made in the fittings in the light of the results already accomplished, which will, without doubt, increase the evaporative economy without reducing the rate of combustion. Compared with the working of boilers by natural draught, the advantages which this system of combustion by air under pressure gives may be summed up as follows:

(1.) Complete combustion of fuel of all qualities, under conditions in which combustion could not efficiently be obtained by natural draught.

(2.) The power of regulating with ease the amount of combustion desired from zero to many times that possible by natural draught, also the capability of maintaining the fuel in the furnace incandescent for a considerable time without appreciable consumption.

(3.) A great reduction in the size or number of boilers required to produce a given power and the capability of increasing the power in steamships far beyond that now attainable with boilers worked by natural draught.

(4.) Greater economy in producing steam from the following causes—(*a*) From more complete combustion of fuel than is attainable by natural draught with a reduced admission of air. (*b*) From the higher temperature of the furnace arising from the more perfect and higher rate of combustion, and from the air supply being partially heated before entering the furnace. (*c*) From the utilization of the waste heat of the escaping gases. (*d*) From the prevention of heat from the furnaces and ash-pits being radiated into the stokehold. (*e*) From the much less expenditure required to supply the air of combustion from a fan than is required to heat a column of air in a chimney to obtain supply by natural draught. (*f*) From preventing the cooling down of the boiler by a rush of cold air to the furnace when a furnace door is opened.

(5.) Less discomfort in stoking, the stokehold being kept fresh and cool by by the radiation of heat from the furnaces being prevented, and the fan drawing fresh air into it continuously independently of ventilators.

(6.) The complete absence of the great nuisance of smoke in the use of steam power.

(7.) The abolition of all unsightly

chimneys in town and country now necessary for combustion by natural draught.

The economic advantage of supplying the air of combustion by mechanical means, instead of by the rarefying of a column of air in a chimney by heat, as in natural draught, is given in the concluding illustration. I close this paper by giving an example of the reduction which (basing on there sults I have already attained) may with safety be made in the number and dimensions of boilers in large ocean steamships. I take, for comparison tne *Oregon*, the latest of the large high-speed Atlantic liners. The published accounts give the boilers as nine in number, each 16 feet 6 inches diameter, length 18 feet, with eight furnaces, or seventy-two in all, each 3 feet 6 inches in diameter. Fire-grate, 6 feet in length, making a total fire-grate of 1,512 square feet. Proposed indicated horse-power, 12,000; assumed consumption of coal per indicated horse-power per hour, 2.6 lbs. Total consumption per hour being thus 31,200 lbs., or 13.92 tons. I have here assumed the consumption per indicated horse-power at 2.6 lbs. per hour, without inquiry, as there might be reasons for her owners or builders withholding this information. It is well known, however, that the high speeds in these large steamers are only maintained by a considerable sacrifice of economy; and I have, on the authority of the managing owners of another high-speed Atlantic steamer of about same size and power, that 2.6 lbs. does not over-state the rate of consumption in their experience. I am not aware that 12,000 indicated horse-power has been actually attained in this steamer at sea, but if it has, it could only be attained by stoking of the most severe character. The enormous space occupied by these boilers and by the coal bunkers can easily be calculated from the above particulars.

Using data already verified by actual trial, the boilers, which, on the system of combustion I have had the honor to bring before you, would easily supply steam to the engines sufficient to develop 12,000 indicated horse-power, would be six in number, 15 feet in diameter, with six furnaces only in each boiler, or thirty-six in all, each 3 feet 9 inches diameter. The fire-grate 4 feet 6 inches long, making an aggregate of 641.25 square feet of grate. The consumption of coal in the engines working from the boilers, I believe I may safely assume as 1.6 lb. per horse-power per hour, seeing there are engines which approach this rate of consumption supplied with steam from boilers worked by natural draft, having a lower evaporative economy. The consumption of coal at this rate is therefore $12,000 \times 1.6 = 19,200$ lbs., being 8.57 tons per hour, or 320 lbs. per minute. In comparing the expenditure incurred in supplying the air for combustion by an engine and fan in the one set of boilers with that incurred by supplying the same by natural draught in the other of these two sets of boilers, I will assume that the air supply to the furnace per lb. of coal consumed is the same in both cases, though the reduction of the air admission is one of the chief objects of my system of mechanical supply. Assuming the air supply to the furnace to be at the very moderate rate of 15 lbs. per lb. of coal consumed, and that the temperature of the escaping gases of the boilers with mechanical supply is 300° less than the escaping gases of the boilers with natural draught, we have now the means of calculating the required expenditure of each form of air supply. This difference in temperature I assume, because it would be realized in practice. One of the essential points in this system of forced combustion is the reduction of the temperature of the escaping gases to a point far lower than would be possible in such boilers worked by natural draught.

Taking first the boiler with natural draught, consuming 31,200 lbs. of coal per hour, or 520 lbs. per minute, we have the weight of air supplied to the furnaces for combustion $520 \times 15 = 7,800$ lbs., to which has to be added the gaseous products of combustion, or the weight of coal less the ash, which, if taken at $7\frac{1}{2}$ per cent, will give $7,800 \times (520\text{-}39) = 8,281$ lbs. as the total weight of gaseous products passing up the chimney per minute, from the combustion of 520 lbs. of coal. With the specific heat of the escaping gases at .246, the total units of heat wasted or expended in obtaining the power of combustion by natural draught beyond that required to supply air by a fan (the temperature of the escaping gases being 300° higher in the former case than in the latter, as explained) are therefore

$8,281 \times .246 \times 300 = 611,137$ units less the equivalent of the power required for the fan supply. The mechanical equivalent in horse-power of these 611,137 units of heat is $\frac{611,137 \times 772}{33,000} =$ 14,296 horse-power. This, of course, supposes the total heat converted into work, and expressed in horse-power units. The actual value of this expenditure of heat is correctly stated in the ratio of the economy of the engines and boilers, which, at 2.6 lbs. per indicated horse-power per hour, is very nearly a utillzation of one-twelfth of the total heat of combustion of coal of average quality; therefore, $14,296 \div 12 = 1,191$ is the actual horse-power equivalent of the 300° heat lost in maintaining the temperature of the funnel in the natural draught boilers.

Coming now to the boilers with the mechanical supply of the air of combustion, and consuming 19,200 lbs. of coal per hour, or 320 lbs. per minute, and taking the weight of the air supplied per lb. of coal at 15 lbs., as in the natural draught boilers, we have $320 \times 15 = 4,800$ lbs. of air supply required per minute for the combustion of 320 lbs. of coal. The volume required at 60°, or 13 cubic feet, per lb., is, therefore, $4,800 \times 13 = 62,400$ cubic feet. To supply this volume per minute from three fans, each having discharge orifices 30″ diameter, or 6.25 square feet area, giving a total area of 18.75 square feet, a velocity of 55.46 feet per second is required, as $18.75 \times 60 \times 55.46 = 62,400$. The horse-power required to supply this weight of air at this velocity per second is found by the usual formula, $\frac{W.v^2}{2g}$. Here $W = \frac{4,800}{60}$ or 80 lbs. air per second, and $\frac{80 \times 55.46^2}{64} = 3,845$ foot lbs., per second, and $\frac{3,845}{550} = 7$ horse-power nearly. This 7 horse-power is the power required to supply the whole air of combustion for 12,000 indicated horse-power, supposing perfect efficiency in the fans, and in the engines that drive them. Assuming 75 per cent. efficiency in the engines, and 50 per cent. in the fans, we have $\frac{7 \times 100}{75} = 9.3'$ and $\frac{9.3' \times 100}{50} = 18.6$ as the gross horse power for supplying the total air of combustion to the furnaces mechanically.

This 18.6 horse-power as against an expenditure equivalent to 1,191 horse-power (required to maintain a temperature in the funnels necessary to give a sufficient supply of air by natural draught to boilers supplying an equal quantity of steam), shows the vastly superior economy of the mechanical supply of air to boilers, if this air is otherwise judiciously used. The reduction in the weight and number of the boilers, of the space occupied in the vessel, and the saving of coal in a vessel of the power of the *Oregon*, and proportionately in other large steamers, by the adoption of the system of combustion I have endeavored to explain, could not fail to effect the commercial character of these large vessels to a very remarkable extent. The considerable increase recently attained in several new large passenger steamers over the highest speeds previously prevailing has only been reached at very great cost. The principal carrying part of the vessels is filled with boilers and coals, and the expenditure in this department in forcing the vessels to their speed is seriously extravagant. To attempt a still higher speed with the present system of natural draught would be commercially ruinous, even if physically possible. The system of combustion in boilers which I have had the honor to bring before you would not only, I believe, permit the highest speed yet attained to be maintained with comparative ease and economy, but would also, I am convinced, allow of still higher speeds being commercially possible.

REPORTS OF ENGINEERING SOCIETIES.

THE INSTITUTION OF CIVIL ENGINEERS.—The Council have awarded the following premiums in respect of the original communications submitted during the session 1883-84:—

For Papers Read at the Ordinary Meetings.

1—A Watt Medal and a Telford Premium to Sydney Walker Barnaby, Assoc. M. Inst. C.E., for his paper on "Hydraulic Propulsion."

2—A Telford Medal and a Telford Premium to Samuel Bagster Boulton, Assoc. Inst. C.E., for his paper on "The Antiseptic Treatment of Timber."

3—A Telford Medal and a Telford Premium to William Foster, M.A., F.C.S., for his account of "Experiments on the Composition and Destructive Distillation of Coal."

4—A Telford Premium to William Tregarthen Douglass, Assoc. M. Inst. C.E., for his description of "The New Eddystone Lighthouse."

5—A Telford Premium to James Atkinson Longridge,* M. Inst. C.E., for his paper on "Wire Gun Construction."

6—A Telford Premium to William Hackney,† B.Sc., Assoc. M. Inst. C.E., for his paper on "The Adoption of Standard Forms of Test-pieces for Bars and Plates."

7—The Manby Premium to George Henry Stayton, Assoc. M. Inst. C.E., for his paper on "Wood Pavement in the Metropolis."

For Papers Printed in the Proceedings without being Discussed.

1—A Telford Medal and a Telford Premium to Thomas Andrews, Assoc. M. Inst. C.E., F.R.S.E., for his paper on "Galvanic Action between Wrought Iron, Cast Metals, and Various Steels, during Long Exposure in Sea-water."

2—A Telford Medal and a Telford Premium to Francis Collingwood, M. Inst. C.E., for his paper "On Repairing the Cables of the Allegheny Suspension Bridge at Pittsburgh, Pa., U.S.A."

3—A Telford Premium to James Henry Apjohn, M.A., M. Inst. C.E., for his note on "The Area of Sluice-opening necessary for the Supply Sluice of a Tidal Canal."

4—A Telford Premium to Thomas Gillott, M. Inst. C.E., for his paper "On the Basic, Open-hearth, Steel Process."

5—A Telford Premium to James William Wyatt, Assoc. M. Inst. C E., for his communication "On the Art of Paper-making by the Machine, as Exemplified in the Manufacture of High-class Writings and Printings."

6—A Telford Premium to William Santo Crimp, Assoc. M. Inst. C.E., for his account of "The Wandle Valley Main Drainage."

For Papers Read at the Supplemental Meetings of Students.

1—The Miller Scholarship to Alfred Richard Sennett.‡ Stud. Inst. C.E., for his paper "On the Electric Light."

2—A Miller Prize to Peter Chalmers Cowan, Stud. Inst. C.E., for his notes on "The New York, West Shore, and Buffalo Railway and the Methods used in its Construction."

3—A Miller Prize to Walter Osmond Rooper, Stud. Inst. C.E., for his account of "Emery Wheels, and Emery-wheel Machinery."

4—A Miller Prize to Richard Moreland (*tertius*), Stud. Inst. C.E., for his paper on "Constructional Ironwork for Buildings."

4—A Miller Prize to Edward Woodrowe Cowan, Stud. Inst. C.E., and a Miller Prize to James Fawcus, Stud. Inst. C.E., for their joint paper descriptive of "A Light-draught Launch."

ENGINEERING NOTES.

A large planing machine in the Charlestown Navy Yard, at Boston, built in 1866-69 by S. Wilmarth, is described by the Boston *Globe* as the largest one in the world. "Its total weight is 300 tons; extreme length, 55 ft.; width, 31 ft.; height, 29 ft. It will plane a piece of metal 18 ft. square and 45 ft. long. It will also plane at right angles or vertically. It will plane a piece weighing 200 tons if required at any given angle, and is capable of boring, turning, slotting, or splining to a depth of 4 ft. and at any taper indicated. The lightest class of work is accomplished with great rapidity and accuracy. Among other specialties it will bore, turn, and cut gear wheels of any required size up to 40 ft. in diameter. It will bore and spline propellers of any size and weight."

The Water Supply of Venice.—Those who have stayed in Venice have learned what it means to be dependent for water upon an army of men, who with barges fetch a daily, or rather nightly, supply of liquid, rarely fit to drink, across the lagoons from a stream emptying near a place called, if we remember correctly, Servola. These old travelers, and those who have yet to visit that much-visited city, will be glad to learn that the inauguration of the Venice Waterworks, by which a real piped-water supply is carried into the romantic city, took place on the 23d of June last, and fully realized the expectations of all concerned in this piece of hydraulic engineering, which is internationally interesting. The contract for the works has been carried out by the Public Works' Company of Italy—Messrs. Breda & Co. including the construction of the reservoir and filter beds at Moranzano on the mainland, and the laying of the pipes under the Laguna to the city of Venice of a total length of 6,460 meters, of a diameter of 80 centimeters—31. 5 in.—and the laying of the total length of mains in the thoroughfares and canals of 25,706 meters of a diameter of 30 centimeters—11. 8 in. In the course of laying these mains they were taken at eighty-five places across canals, and twice across the Grand Canal, work which involved considerable difficulties. The work also included the construction of an engine house and reservoir at St. Andrea, the erection of a pair of 50 nominal horse-power engines, together with laying on the water to all the principal buildings and hotels in the city. The work was commenced early in January, 1881, and was consigned to the entire satisfaction of the concessionaires on the 23d of June, 1884. The concession was originally granted to Mr. D. C. Dalgairns, C.E., of Palermo and Penge, in 1876, upon provisional plans and studies then deposited, and the works have been carried out on the definitive plans presented by him on the 23d of June, 1877. The company to which the property now belongs is the Société des Eaux pour les Etrangers, of Paris, which was formed for the purpose of carrying out this scheme in January, 1879. Probably no city in the world had more urgent reason to obtain and maintain an excellent water supply than Venice. Venice lives upon the pleasures of all the nations of the world, and she could not afford that the fearfully bad supply upon which she has depended from time immemorial should drive away even a few hundreds of the many thousands of her visitors. She has therefore gone to the Brenta for water, and now her visitors may at any time, instead of occasionally, have a glass of water to drink, or may even have a bath. Count Dante Serego degli Allighieri, the Sindaco of Venice—a descendant of Dante—through the medium of G. G. Maranzoni, of Venice, has

expressed his deep sense of gratitude to Mr Dalgairns on the successful inauguration, and adds, "The city of Venice is truly alive as to its indebtedness."—*Engineer.*

IRON AND STEEL NOTES.

R. F. Mushet on Blowholes in Steel and Cast Iron.—The creators of the age of steel have ransacked creation to find methods of preventing or lessening, or altogether stopping, these blowholes, with more or less success, but with a great deal more of failure. As Mr. W. H. Fryer has justly observed, these cells are formed or inflated by hydrogen gas. Take away now that gas, and you will take away the cells; but as long as blast furnaces and Bessemer converters are blown with damp atmospheric air, the hydrogen and the blowholes will more or less be there. Why not dry the air, and try the matter fairly? The drying can be cheaply effected for, I suppose, 3d. or 4d. per ton of metal; but nobody appears to have tried the process yet, although it has been before the public, I think, for over two years. It is true that under the withering blight of that enormous sham called "free trade" iron and steel makers have so much to do to hold their own, and to grind down workmen's wages to the lowest point, that they have not time or money to spare to pay attention to improvements, however valuable and useful they may be. The dry blast, however, is well worthy of a fair trial, both in the blast-furnace and in the Bessemer converter. Iron and steel can only get their hydrogen from the blast, and if that blast is desiccated, no hydrogen can be present to form cells. To take pig-iron made—as all of it is at present—with damp blast, and blow it in a Bessemer converter with dry blast, would not be conclusive, for both the blast-furnace and the converter should be blown with dry air, so that no hydrogen could possibly be present.

I have said that the drying is inexpensive, say 3d. per ton; but I had forgotten for a moment that even one penny per ton is a serious amount under our free trade prosperity, which is eating like a cancer into every manufacture, cutting down profits to zero, and, therefore, annihilating capital and wage-paying power. There is a ludicrous phrase, dear to the soul of the Cobdenite. "Holding their own" is their panacea. If anyone can hold their own, that is, just struggle on, short of a composition or bankruptcy, the Cobdenites record it with pleasure. Formerly, employers of labor looked for profits; now, if they can "hold their own," they must be content. The very colliers are not safe from the devouring maw of free trade, for coal is being imported! Even the poor colliers have to hold their own, if they can. When will the petrified fossillized generation of Cobdenites pass away, and so give home industries a chance to thrive once more? Free trade has desiccated home industries, and given the bread of the British workmen to foreigners.

To Tell Iron from Steel, in Small Pieces.—A new fracture ordinarily furnishes the means for classifying test pieces, but its appearance is not a sufficiently safe guide in dealing with good, fine-grained iron or very soft steel. In order to effect the separation with ease and certainty in such cases, Walrand has given a simple method in the *Memoires de la Societé des Ingenieurs Civils*, 1883, page 531. It is by observing the fracture of the test piece after heating and allowing it to take a blue color. The trial can be conducted in the following manner. Take a test piece about twenty-five or thirty centimeters long and make a slight scratch about four or five centimeters from the end. Then heat one end slowly and uniformly to a dark red color (325° to 400° C.) and cool it in water. During the cooling, while the piece is still warm, it must be rubbed with a file from time to time, until the shining metallic surface laid bare, has assumed a dark yellow, or better, blue color, when it is to be cooled quickly and completely. The fractures of the piece broken at the mark serve for comparison. Ordinary wrought-iron broken when cold appears fibrous or crystalline; but, treated as above, its fracture is dull, irregular, and of short fibre. Hard and moderately hard steel are fine grained; after the heating and subsequent treatment, they have a shining, totally or partially, smooth fracture. Swedish iron has only traces of fibers and is hardly to be told from soft steel; after treatment the fibers become distinct, the smooth appearance is lost, and the iron becomes so much the more distinguishable from soft steel treated in the same manner.—*Dingler's Polytechnisches Journal.*

Tests of Iron and Steel Girders.—Some very accurately conducted experiments were recently made by the Dutch government to test the relative strength of iron and steel girders. Hard and soft steel girders were put in comparison with iron girders of a good quality. The tests were made by specimens of the materials from which the girders were made, and also by actual finished girders. Each of the steel girders showed a large increase in strength over the iron girder. The soft steel girders proved 22 per cent. stronger, and the hard steel girders as much as 66 per cent. stronger. The greater strength of the soft steel over the iron in the specimens submitted was fully attained, and even exceeded, in the girders. The hard steel girders did not show so large a percentage of greater strength over the iron girder as did this material in the specimen over the iron in the specimen. This, however, may be accounted for as the result of punching the rivet holes without reaming, for a girder with punched holes gave way during the trials by a fracture of the tension flange, whereas a girder with reamed holes gave way in the compression flange, and probably would have stood more before fracture had taken place in the tension flange. The latter girder did not appear to bear truly upon its supports, and it was probably this which caused it to fail in the top flange when it did. Punching rivet holes without reaming did not produce any results other than an apparent loss of strength, as compared with reamed holes. The trial pretty well established that the strength of steel girders, strained as these were, is about the same for the two flanges if they are made alike in section.—*Iron.*

RAILWAY NOTES.

It is stated on official authority that there are now 3,000 miles of railway in operation in Brazil. There is scarcely a province bordering on the ocean that has not one or more railways, and all lead to the west, or toward the interior. In the most southerly province, Rio Grande do Sul, the Porto Alegre Railway, now open 91 miles to Cachœira, is being built by the State, and partly for strategic purposes. Steady increase has been lately made in extending several lines; new lines have also been commenced, and there is reason to suppose that the development will annually increase, though the broken surface of Brazil generally, and especially the mountains near the sea-coast, are great obstacles to the rapid development of railway construction. None of the railways have yet penetrated to the vicinity of wild or public lands. Some of them traverse extensive areas of uncultivated land, but as yet no grants of land have been made in aid of railways. The Brazilian Government, however, has extensively guaranteed the payment of interest on railway capital. Its annual burden for the payment of such interest amounts now to a very large sum. The rails for all the lines have to be imported, and the greater part are purchased in England. Many of the locomotives are imported from the United States. It may be also mentioned that several American civil engineers have gained distinction by their services in Brazil, but the field now appears to be occupied by native talent. At the same time, several English railway companies naturally employ English engineers.—*Engineer.*

A great Railroad event in South America.—A great event in Buenos Ayres has been the recent completion of the Andine Railway to Mendoza, capital of the province of that name, and lying at the foot of the Andes. The importance of this event cannot be exaggerated; the line, it may be said, crosses the continent, stretching from the Parana to the Cordillera. A zone of immense natural wealth is thus thrown open by this quick means of communication, and the traffic on the line promises to be very large. Mendoza is one of the richest provinces of the republic; it covers an area of about 5,000 square leagues of land at the foot of the Andes, with a population estimated at 150,000 souls. The rivers Mendoza, Tunuyan, Desaguadero, Diamante, and Atuel irrigate over 1,000 square leagues of land, and the soil is so bountiful that the yield is often a hundredfold. A very active trade is carried on in the export of fat cattle to Chili, but the great industry of the present and future is viniculture. The Mendoza wines are well known; the production is doubling every two years. The great drawback of the past—onerous and difficult means of communication—is now removed, and the development of all the industries of the province may be looked forward to. The mineral wealth of the province is said to be beyond calculation. —*Iron.*

Railway Accidents.—A blue book has been issued, giving returns of accidents and casualties as reported to the Board of Trade by the several railway companies in the United Kingdom during the first three months of the present year, together with reports of the inspecting officers of the railway department to the Board of Trade upon certain accidents which were enquired into. Among the accidents reported were 10 collisions between passenger trains or parts of passenger trains, by which 120 passengers and four servants were injured; 14 collisions between passenger trains and goods or mineral trains, by which 13 passengers and four servants were injured; nine collisions between goods trains or parts of goods trains, by which two servants were killed and seven injured; 16 cases of passenger trains or parts of passenger trains leaving the rails, by which two servants were killed and four passengers and four servants injured; and 32 cases of trains running over cattle or other obstructions on the line, involving injury to four passengers. The total number of persons killed on railways in the United Kingdom in the course of public traffic during the past three months was 254. The injured numbered 990. This is a decrease as compared with the corresponding period in 1883.—*Engineer.*

ORDNANCE AND NAVAL.

Compound Armor Plates.—The Danish Government, following the example set by England, France, Italy, and other maritime powers, has decided in favor of the adoption of English compound armor plates, as superior to all others for the protection of ships against modern rifled ordnance. This was only to be expected. It would, indeed, have been surprising to find a government such as that of Denmark arriving at any other conclusion, after the experience gained at the trials which have been made throughout the world of iron, steel, and compound armor plates. The latter have invariably proved superior to all other means of protecting a ship's side, and, although it was at one time thought that the influence in Copenhagen of the French Creuzot works was powerful enough to influence the Danish government to use solid steel plates to protect their ships, the final decision arrived at last week shows that the Danes are determined to use the very best material they can procure, irrespective of its nationality. It will be remembered that at the last trials on the island of Amager, near Copenhagen, both the Ellis and Wilson systems of facing iron plates with steel were tried. The Danes have now decided to adopt the latter system, as being, in their opinion, superior to the former. It has come to be admitted by all naval powers that compound armor is far better able to withstand the heavy blow of the projectile thrown by modern ordnance than are either solid steel or iron plates; and, since steel-faced plates are solely made in Sheffield, this English industry will be busy for many years to come. Russia, however, is alive to the advantages of producing at home all the material required for its navy; and Messrs. Cammell & Co. have recently received an order from the Russian government to forthwith set up works within its frontiers for the manufacture of Wilson's compound armor plates. The armor for the French ships Duguesclin, Vauban, Requin,

Caiman, Admiral Baudin, Dupere, Furieux, and Tonnerre, is being made in France on the Wilson system. That of the Indomitable was manufactured in Sheffield by Messrs. Cammell & Co.; as were also the plates for the Russian Vladimir Monomach, the Chinese Chen-Yuen, the Braziliac Riachuelo, the Italian Italia, and many other modern ships of foreign governments. Our Admiralty use both Wilson and Ellis compound plates, it not being deemed expedient, where so many ships are constantly in course of construction, for the government to be solely dependent on one firm.—*Army and Navy Gazette.*

MACHINE GUNS IN THE FIELD.—At the meeting of the Royal United Service Institution, held on July 4, Capt. Lord Charles Beresford, R.N., read a paper on the above subject. There were several specimens of those weapons on the floor of the theatre—a Gatling, with ten barrels, worked by means of a crank; a two-barreled Gardner, and a couple of Nordenfelts, one of them mounted on a carriage complete.

Lord Charles Beresford said that, as a naval officer, he had a certain amount of hesitation in taking up a question which officers of the army might naturally think belonged peculiarly to themselves. At the same time, it must be remembered that the navy had had more actual experience in the working of machine guns in the field than any other portion of Her Majesty's service, and guns for this purpose were supplied to the navy, but not to the army. A machine gun proper was a gun without recoil, or, in other words, was a weapon which did not require re-laying after every shot There were two entirely distinct kinds of such guns; the one a shell-firing gun, and the other a bullet-firing, or rifle-caliber, gun. International law did not admit of any explosive projectile under 14 ounces in weight, and the weight of a machine gun throwing such a projectile would detract from its value as a machine gun, and would make it almost artillery. Dealing with the small-bore caliber gun, to which he should confine himself, Lord C. Beresford discussed the recent trials at Poona of a ten-barreled Nordenfelt, and showed that the results were equal to the fire of seventy men with rifles, and 80 per cent. better than the hits from four 7-pounder mountain guns. He pointed out the value of the guns to the landing party at Alexandria in 1882, and proceeded to demonstrate the value of the weapon to artillery, cavalry, or infantry. He quoted the words of eminent military men in all branches of the service, giving some cases where guns had been of value, and others where days would have been saved and reputations upheld by the presence of a couple of machine guns. An instance was at Maiwand, where, he said, if there had been any machine guns the guns would never have been lost, the cavalry would never have been kept so many hours inactive under a heavy fire, and the day would have been saved. He described the machine guns mounted on galloping carriages, and mentioned a few engagements in which the English troops had been fighting where a machine gun mounted as described would have been of infinite service. He urged that it would be better to do away with the limber altogether, and commented severely upon the disaster likely to result from a mixture of ammunition, quoting an instance where a box of Gatling ammunition got mixed with the ordinary ammunition, and where if the men had attempted to use the cartridges, the result would have been that every rifle would have been virtually spiked, as the discharged cartridges could be got out only with great difficulty. In conclusion, Lord Charles Beresford said that the machine guns of the present day are accurate, rapid in their fire, and terrible in their man-killing power is beyond doubt; whether they can or cannot be better utilized for the services is a grave question for consideration. For volunteers they would appear to be admirably adapted, and the London corps under Colonel Alt have lately demonstated (as far as drill can do, and in the recent sham fight at Portsmouth) how exceedingly useful they might be. Artillery officers, as a rule, are supposed not to look upon them with favor, but most of the arguments used against them appear to treat them as guns instead of clusters of rifles, which they really are. In the course of a discussion which followed, Admiral Boyes expressed the opinion that it was a *sine qua non* that the ammunition for machine guns should be the same as for guns used in the field. Captain Colomb remarked that the navy, to a man, considered that the age of the machine gun was just commencing. Major Lewis referred to instances where single-barreled machine guns would be most useful. Other officers also took part in the discussion.

BOOK NOTICES

PUBLICATIONS RECEIVED.

TEMPERATURE OF THE ATMOSPHERE AT THE EARTH'S SURFACE. By PROF. WILLIAM FERREL. Washington: Government Printing Office.

L' Assainissement Suivant le Système Waring. Paris: Baudry et Cie.

Papers of the Institution of Civil Engineers:

No. 1927. The Canadian Pacific Railway. By J. C. James and Allan Macdougall.

No. 1988. The Alleghany Suspension Bridge. By Francis Collingwood, M. Inst. C.E.

No. 1966. Wandle Valley Main Drainage. By William Santo Crimp, Asso. M. Inst. C.E.

No. 1958. Wire-Gun Construction. By James Atkinson Longridge, M. Inst. C.E.

No. 1968. Hydraulic Propulsion. By Sidney Walker Barnaby, Asso. M. Inst. C.E.

Proceedings of the Engineers' Club of Philadelphia.

FORESTRY IN NORWAY. Compiled by JOHN CROUMBIE BROWN, LL.D. London; Simpkin, Marshall & Co.

The student of Physical Geography will find much to interest and to instruct in this little volume. All the conditions of climate and soil that influence the growth of vegetation are fully discussed.

How completely the physiography of Norway is presented, may be inferred from the following list of chapter headings; General features of the country; Forest Scenes; Mountain Plateaux; Distribution of trees in Norway; Con-

ditions upon which distribution depends; Conditions of distribution in Norway; Temperature; Rainfall; Rivers; Lakes; Winds; Geological Formations: Mountains and Fields; Snow fields and Glaciers; Mechanical action of Glaciers: Saeter life; Valleys, Forest Exploitation, and the Timber Trade; Shipbuilding and Shipping; Forest Devastation: Remedial Measures.

This is one of the lesser contributions to the literature of Forestry from the pen of this industrious writer, but it will be widely read for the information it affords on other topics.

GUIDE DER NIVELEUR. Par J. VERRINE. Paris: Paul Dupont.

The different methods of leveling, from the rudest to the most finished, are elaborately described in this little book.

The instruments are classified as of three kinds, the builder's level and the old water level representing the first class; the pendulum level and the reflecting level, the second class, and the engineer's telescope level the third class.

Two full chapters are devoted to the different forms of rod and the errors to which they may respectively give rise.

Section and cross-section work with notes, profiles and graphic solutions of earthwork problems form the matter of the four conclusive chapters.

Nearly two hundred illustrations are interspersed throughout the text.

A TREATISE ON PRACTICAL MINE VENTILATION. By EUGENE B. WILSON. New York; John Wiley & Sons.

The author of this little treatise explains in his preface that he "has endeavored to deal with ventilation in such a manner that no one with a fair knowledge of the English language and of arithmetic need despair of thoroughly mastering it.

"Knowing that the miner possesses but a comparatively small stock of words, and is not an adept in algebraic formulas, the writer has taken a different position from the standard works on the subject, endeavoring to do away with abstruse language, and such highly mathematical formulas as are only calculated for well educated engineers."

The omission of abstruse language has been fairly well observed, but in some instances at a large expense of accuracy. For example, the author says of oxygen, "It devours everything with which it can unite; it corrodes metals, decays fruits, promotes combustion, and is a prime necessity for health.

"The body is a stove in which fuel is burned; the chemical action being the same as in any other stove."

Again on page 25 occurs the following; "The 'motive column' is a 'head of air" of such a height that it will equal the difference between the weight of the downcast and upcast columns of air."

This is followed by an incorrect formula:

$$M = D \times \frac{t - t_1}{459t + t_1}$$

The explanation is furthermore afforded that —"The relative diameters of the shafts make no difference upon the total pressure, so far as the considerations regarding ventilation are concerned. This is termed the 'pneumatic paradox.'"

All that is not abstruseness in this last paragraph is misinformation. It should, however, be stated in justice to the author that the illustrated examples serve in most cases to set the reader right, and would prevent the learner, if he followed the arithmetical examples, from serious practical mistakes.

A faulty definition, however, may lead the student astray to his future disadvantage. Such an one appears on page 47:

"Coefficient of friction is a term used to represent the constant resistance met with by air during its journey through the mine."

The typography and the arrangement of the formulas are exceedingly good.

SORGHUM: ITS CULTURE AND MANUFACTURE, ECONOMICALLY CONSIDERED AS A SOURCE OF SUGAR, SYRUP AND FODDER. By PETER COLLIER, Ph. D. 8vo, cloth, Cincinnati, R. Clarke & Co.

The development of an important industry is an achievement deserving of great honor, and so when an American chemist establishes on a solid basis a new American industry, unqualified praise is due to him.

In 1877, at the request of the head of the Agricultural Department at Washington, D. C., Professor Collier undertook the examination of Sorghum and maize as sugar-producing plants. Since that time much of his ability and energy has been devoted towards the practical solution of these questions, with special reference to Sorghum. The different annual reports of the chemist give the results of careful investigation and show a steady progress in his work. Experiments were made for the purpose of determining the best varieties of the plant for cultivation, the conditions of soil, the effect of fertilizers, the most suitable forms of machinery. These and other like topics came up for consideration and were successively studied. Before long, factories were established at various localities throughout the country. Certain of these were unsuccessful while others flourished until, in 1882, the statistics given in the annual report of the Agricultural department for that year show a production of 510,780 lbs. of sorghum sugar. In 1883, upwards of 850,000 lbs. were reported, and to-day probably more than 1,000,000 lbs. are being manufactured. According to the census returns of 1880 the production of sorghum syrup is given as 28,444,202 gals., while in 1860 only 6,749,123 gals. were obtained. Such, in brief, is the history of the development of sorghum sugar industry.

In the book before us, we find arranged in the most admirable order, a discriminating and judicious selection from the information which Prof. Collier has accumulated from his experience and researches. In his own words it includes "the most important facts relating to the economic production of sugar, syrup, and fodder from sorghum." As an authority on this subject, the author stands second to none. He is an enthusiast, and as he is conscious of the truth of his statements, conviction that he is right will follow from a perusal of them.

Prof. Collier has done much to benefit his fellow citizens, he has also suffered severely from adverse criticism. It will be the mission of this book to widen and extend his influence, to increase his reputation, and to bring about a full realization of his most sanguine hopes for the sorghum industry.

TEXT-BOOK OF DESCRIPTIVE MINERALOGY. By HILARY BAUERMAN, F. G. S. London: Longmans & Co.

We have here a descriptive treatise on mineralogy, though only the more important species, scientifically or economically speaking, are described in detail.

The subject of nomenclature is considered at some length. The author remarks that "names founded on physical or chemical peculiarities are, as a rule, among the most uncouth and inconvenient, and as they are not unfrequently founded upon false analogies, some trivial character, or one that may be common to many other minerals being selected as a specific distinction, a knowledge of their etymology is not always of advantage as an aid to the memory." Mr. Bauerman holds, very judiciously, that in the case of minerals worked as ores the ordinary commercial names should always be used where possible. "Thus for all purposes copper-pyrites, tinstone, and zinc-blende are preferable to calkopyrite, cassiterite, and sphalerite."

The author's classification is substantially the same as that followed in the second edition of Rammelsberg's work. The statements of localities are generally on a level with the most recent discoveries. The only mineral of commercial importance which we miss is bauxite, which certainly does not figure in the index. The illustrations, being printed from the blocks used in Brooke & Miller's work, are excellent, and contribute greatly to the value of the work. *Chemical News.*

STEAM AND MACHINERY MANAGEMENT. By M. POWLS BALE, M. I. M. E., ASSOC. M. I. C. E. London: Crosby, Lockwood & Co., 1884.

This work forms one of Weale's Rudimentary Series published by Messrs Lockwood & Co.—Owing to the great and increasing competition of the present day in every branch of manufacture, it cannot be denied that it becomes increasingly necessary to exercise every economy that will either lessen the cost of production or increase the efficiency of the plant employed. It must be admitted that a considerable loss often arises through inefficiently managed or badly arranged machinery; and this subject is now attracting a considerable amount of attention, with the result that many old manufactories have been remodeled and new ones built and arranged with appliances to reduce manual labor and the cost of production to the lowest possible limit. As a portion of the matter contained in the author's recently published work, *Sawmills: their Arrangement and Management*, would, with modifications and by the addition of other chapters, be made more or less available to general machinery users, and be issued at a less cost than that work, he has with this object in view especially written some fifty pages, and issued *Steam and Machinery Management*. The variety of machinery and manufactures being so great, the author has only selected a few kinds; but most of the hints being general, and not arbitrary, they may be modified as circumstances may dictate. What has now been written by the author may be found of some use by those who intend erecting machinery or who have it already in operation.

MISCELLANEOUS.

IN the last paper of the session read by Mr. Stayton, C. E., at the Institution of Civil Engineers, the aggregate length of the streets of London was given as 1,966 miles, of which, excluding 248 miles in course of formation, 1,718 miles are thus maintained by various authorities, namely: Macadam, 573 miles; granite, 280 miles; wood, 53 miles; asphalte, 13½ miles; flints or gravel, 798½ miles. The existing area of wood pavement is 980.533 square yards, and its estimated cost £600,000. Not more than 4.38 per cent. is east of the city or south of the Thames.

AUTOMATIC LIGHTING OF BEACONS.—In America a system of automatic beacon lights has been adopted. Each beacon is furnished with a reservoir of sheet iron, containing gas under a pressure of fifteen atmospheres. The quantity is sufficient to light the beacon for three months; and fresh supplies are periodically delivered by a vessel which conveys the gas from the factory. A clockwork installed in the beacon, turns on, and lights the gas at the hour fixed for this purpose. The experience of several months has served to test this plan and it has proved so far successful. Attendants live on the shore near the beacons, and see if they are working properly.

PROFESSOR G. FORBES has made some observations on a magnetized chronometer watch. The watch went slow several minutes a day. He found the rate to vary with the position of the watch with respect to the cardinal points and also in a vertical plane. He traced the variation of rate to magnetization of the balance spring, the bar and the screws. The fact that it varied with position suggested that a magnetized ship's chronometer might be made which would integrate the course and give a mean course. Messrs. E. Dent & Co. had since fitted a gold spring and a platinum iridium balance to the chronometer, and rendered it non-magnetizable.

A SURVEY of the coal lands of the Canadian North-West has just been made under the directions of the Dominion Geological Survey. Approximate estimates of the quantity of coal underlying a square mile of land in Bow and Belly rivers district give in one case 4,900,000 tons, in two cases 5,000,000, and in another 9,000,000 tons. The coal is, in general, exposed on the surface, and there is consequently little labor necessary to the working of the mines. Though no Government surveys have been made in the surrounding districts, coal-bearing strata are known to extend to the north and west of the parts from which coal is now being taken.